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OF THE

INSTITUTION OF ELECTRICAL ENGINEERS,

INCLUDING

ORIGINAL COMMUNICATIONS ON TELEGRAPHY AND ELECTRICAL SCIENCE.

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AND EDITED BY

W. G. McMILLAN, SECRETARY.

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VOL. XXX. 1900. NO. 147.

An Extraordinary General Meeting of the Institution was held, in association with the American Institute of Electrical Engineers, at 9.30 a.m. on Thursday, August 10th, 1900, in the United States National Pavilion in the Paris Exhibition, Professor John Perry, President of the Institution of Electrical Engineers, and Mr. Carl Hering, President of the American Institute of Electrical Engineers, presiding.

MR. CARL HERING: It gives me great pleasure to open this meeting, the first one held jointly by the Institution of Electrical Engineers and the American Institute of Electrical Engineers.

Such international meetings between societies whose aims are alike but whose surroundings are different, cannot fail to be of interest and benefit to all who take part, and to draw closer together the different nations that participate, thus strengthening the ties which bind together the electrical engineers of different countries.

The promising success of the present session gives us reason to hope that it will soon be followed by other similar international meetings of electrical engineers.

In behalf of the members of the American Institute of Electrical Engineers, I wish to thank you, Mr. President and members of the Institution of Electrical Engineers, for the great pleasure you have given us by joining us in this international gathering. It was quite a venture for us to hold a meeting so far away from home; in fact, when I first suggested it some years ago the proposition met with some ridicule by members of our board of managers. When we found, however, that our older and larger sister society in England joined us so heartily in our proposition, the success of our venture was at once

assured. I feel sure that those of our members who are present, and many who are not, hope that this, our first joint meeting, will be the beginning of a series of similar meetings of our sister societies, and that the next one will be in the home of our Institute.

(Mr. Hering then made a few remarks in French and in German, welcoming the guests from other foreign countries, and expressing the hope that the electrical societies of the leading countries will join in holding similar meetings, the next of which he hoped would be in the United States.)

In accordance with the arrangements of our joint Committee, there will be two presiding officers. Professor Perry, as the senior officer, will conduct the affairs of the meeting, and will now address you.

Professor PERRY, F.R.S.: After a very few words from me we shall commence the discussion. I can only say to the Americans who are now our hosts in this building that their visit to England gave us very great happiness indeed for four days. I think that every Englishman who joined in the parties feels that it did really give us more pleasure than it was possible for our guests to experience.

There are no minutes to be read at the beginning of this meeting because there have been no such meetings in the past, but minutes are being prepared of this discussion, and let us hope that they will be read on a future occasion. In the name of the two Institutions of Electrical Engineers, we welcome you, ladies and gentlemen, to this meeting.

I will now ask M. Mascart to say a few words.

M. E. MASCART: Je dois d'abord adresser à la Société des ingénieurs électriciens de Londres mes très vifs remerciments pour l'honneur tout à fait exceptionel qu'elle a bien voulu me faire en me nommant vice président: j'ai été extremement touché de cette marque particulière d'estime, et j'ajouterai d'affection, de la part des membres de cette société.

On a émis tout-à-l'heure la pensée qu'il serait très utile de rendre plus fréquentes les réunions analogues à celle ci, c'est-à-dire des réunions comprenant les membres des sociétés d'électricité des nations les plus importantes; vous avez commencé par donner l'exemple, et nous vous remercions d'avoir réuni à Paris les membres des deux grandes sociétés d'Amerique et d'Angleterre.

nous conviez à aller aux États Unis; je vous assure serait pour moi un très grand plaisir, parceque j'ai é un souvenir charmant du voyage que j'y ai fait il ques années, mais je commence à être de ceux qui gent plus beaucoup. J'espère, néanmoins, que les s de la société des électriciens, qui se sont déjà pés de cette question, répondront à votre appel bientôt, vous pourrez tenir en Amérique, à Phila- par exemple, une réunion des sociétés des électri- Amérique, d'Angleterre, de France et d'Allemagne mpter les autres pays.

si je ne puis pas être des votres dans cette circons- e vous accompagnerai, cependant, de mes voeux les ncères pour le succès de votre réunion.

intenant, au nom des électriciens français, au nom de été des électriciens, je vous remercie de nouveau et ordialement d'avoir choisi la ville de Paris pour y tenir séance de vos sociétés associées. Je remercie, en par- ier, nos deux honorables présidents qui ont eu l'initia- féconde de cette réunion extra-territoriale.

Professor J. PERRY: I will ask Mr. Ferranti to open the cussion.

SCUSSION ON THE RELATIVE ADVANTAGES OF ALTERNATING AND CONTINUOUS CURRENT FOR A GENERAL SUPPLY OF ELECTRICITY, ESPECIALLY WITH REGARD TO INTERFERENCE WITH OTHER INTERESTS.

Mr. S. Z. DE FERRANTI: I am indeed greatly honoured by being llowed to open the discussion at this most fortunate meeting. I say most fortunate, because you will all agree with me that nothing could be better than that the American and English Associations of Electrical Engineers should have a joint meeting. There is only one thing in my mind which is perhaps better, and that is that we should be holding this meeting in Paris, and thus show what a great tie really unites us all, and how much more friendly our feelings really are than is often supposed. The subject for this discussion was left for the English Institution to select, and I can assure you, gentlemen, that it was a matter of no little difficulty to decide upon what we should discuss. The subject which has been chosen you may at first think is simply a revival of the old contention between alternating and continuous current. I hope, however, that it is no such thing. Matters have greatly changed since the early days when the advocates of each of the two systems were, I may almost say, bitter enemies, and when they thought that everything that was done by the other side was

Mr. Ferranti.

Mr. Ferranti.

wrongly done. We have all found that that is not so, and that there are great merits in the particular uses of both systems. But, as I have said, things have changed, and they have not changed yet as much as they are going to change in the future. The question that we have to deal with must be considered in the light of what electricity will be ten, fifteen, or twenty years hence. You have seen how the industry has grown; you know, by being members of the electrical profession, how much it has done up to now. I think few of us, although it is our life's work, realise what electricity is going to grow to, and how important and universal a part it is going to play. It is, however, in this light that we must consider what are the best lines to be worked upon. It is no longer the small isolated systems which could be worked on one plan or another according to convenience; that we have to deal with, but it is the question of transmitting and using big powers all over large areas. Now, with regard to the advisability or desirability of one system, the continuous as against the alternating current: many of you know that I have consistently worked with alternating currents, and you will therefore appreciate that my knowledge of the other subject is not what it should be as compared with that of those who have worked more exclusively in that direction. I, however, desire to dissociate what I have done from what I am going to say, and rather than give so much my own opinion, I shall suggest a few points to you bearing upon what the general opinion is on this subject, as I consider it most important.

Looked at from the new point of view, this is what presents itself: Which will be the possible system in the future? I say "possible" because many of us have had very serious experiences of the electrolytic effects which are produced to such a large extent by the continuous current. What I am wondering is this: If this system is developed and extended to a very large extent, will it be possible satisfactorily to preserve any metal work in the earth at all? We have not only got to deal with the lead coverings of electric cables, but we have also any amount of structural ironwork—water mains, railway rails, gas pipes, water pipes, and all sorts of things—the quantity of which is increasing every day in our streets all over the country. The amount of property invested underground in one or other of these forms is reaching a very great figure; and it becomes a serious question for us, not so much in the light of to-day, but in the light of the future, when immense developments will have taken place, to know on what lines we should go, and how best to protect this immense property, which may be injured by the otherwise beneficial work which we are doing. Of course alternating currents are not quite free from the liability to produce some harm, but what little harm they can do is not of commercial importance. Therefore I consider it will want our most careful thought and work and investigation to find out really how we can diminish these electrolytic troubles, due to continuous currents, or whether it will not be necessary to substitute alternating current, which produces but slight detrimental effects, and see how to overcome what disadvantages are now left to it. I hope that a great many of you gentlemen present will give us valuable information on this subject, and, after you have left this meeting, having

heard the discussions which have taken place, that during the next few years you will give the matter your careful consideration, and then contribute your views through the electrical press, or by another such meeting as this, to the electrical profession at large. Mr. Ferranti.

Again, gentlemen, I must thank you for having been allowed to open this discussion. I hope it will be a full discussion and that what I have said may stimulate discussion and lead to careful consideration of the subject which will prove beneficial to the electrical industry in the years to come.

Mr. Arnold. Mr. BION J. ARNOLD: I wish to express my appreciation of the honour conferred upon me by being permitted, in this discussion, to follow so distinguished a worker in the electrical field as Mr. Ferranti. I did not know about my colleagues' intentions of calling on me this morning until late last evening, otherwise I would have taken time to prepare my remarks. We have all heard that the man who dares to assert that "electricity is in its infancy" is liable to annihilation on the spot; but after Mr. Ferranti's remarks I fancy we may be considered, for the moment at least, to be on the threshold after all.

I think it may bring out the discussion of this subject between the different countries, here represented, more thoroughly if I outline briefly the practice in the country which I come from, viz., the United States of America. For lighting work we started in our large cities with the direct current system as advocated by Mr. Edison. By the way, I once saw a letter from Mr. Edison, written some ten years ago, in response to an inquiry addressed to him asking his views on the relative advantages and disadvantages of the alternating and direct current. His letter intimated that he thought the alternating current was of Satanic origin, that it was a delusion and a snare, and that no good could come out of it. Mr. Ferranti has proven that the contrary is the case, and judging from his remarks and the work of many others it seems as though the ideas of his Satanic Majesty may yet prevail.

Returning to our subject, we started in our large cities with the direct current system and in our smaller cities with the alternating system, To-day the direct current seems to have not only held its own in our large cities, but, in addition, is replacing the alternating system in some cases, although the alternating system holds its place for smaller cities covering widely distributed districts and for transmission to the outlying districts of our larger cities. In the City of Providence, Rhode Island, one of the first and leading cities in the electric lighting industry, the alternating apparatus originally installed has been entirely thrown out, in the underground districts, and replaced with a direct current system of supply, delivering energy on a three-wire 480–240 volt system. The same thing is taking place in the City of Saint Louis, Mo., although at this point they have not abandoned the use of the alternating apparatus, but a competing company has installed a direct current system on the 480–240 volt three-wire system, which company has largely taken the business of the alternating company within the district it reaches, and the net earnings and operation of the direct current company have been very satisfactory. I mention these two important installations to emphasise my statement that the direct

Mr. Arnold. current is holding its own in our larger cities. I believe the alternating current will hold its prestige in our smaller cities and in the outlying districts of our larger cities.

In answer to Mr. Ferranti's advocation of the alternating current for lighting work in large cities, I will say that there are no means, so far as I know, for equalising the load upon the power station when the alternating current is used, thus losing the advantages gained in direct current work when using storage batteries. The installation in the latter case would require less investment than would be necessary if the alternating current were used, assuming a reasonable area over which the energy is to be distributed.

Coming now to railroad work, we started with a 500-volt direct current system, and gradually increased to 600 and 700 volts, and are now utilising the latter on the sections of the roads contiguous to the power houses, and driving our more distant sections with three-phase 25 or 30 cycle alternating current. I believe this will be modified, and that the alternating current will ultimately predominate in railway work. I believe that we shall soon eliminate entirely the sub-station, the rotary converter, and possibly the static transformers at the power house. We shall at least eliminate the sub-stations. Mr. Ferranti is no doubt working on this theory, and so also are others, in the respective countries here represented, including some from the United States. Some of us feel that there is a fair hope for success. It may, of course, be slow in coming, but that it will come I have no doubt. There are two main difficulties at present in the way of a complete and successful alternating current railroad system. The first is the lack of a practical method of starting and controlling the speed of an alternating motor without excessive consumption of energy. The second is the one pointed out by me when referring to lighting work, viz., the lack of a load equaliser corresponding to a battery in direct current work. Some of us are hoping for and endeavouring to develop a system by means of which we will utilise or have the advantages of an equalising reservoir as it were, thereby taking the place of the storage battery in direct current railway work. I believe this is coming, and if it is successful we shall have an ideal system for long-distance railway work, utilising all the advantages of the alternating current for transmission and the advantages of the equalised load factor for economy, and we shall not have the disadvantages of the sub-station and its corresponding maintenance and labour expense.

I do not know that I can add anything more to the discussion, but if I have succeeded in pointing out, in a general way, the lines upon which the engineers of our country, and possibly of others, are working in the lighting and railway fields, it is all I can reasonably hope to do in the time allotted to me. If any further information is desired regarding the details of the work to which I have alluded, I will, if the questions are asked, give such information as I can consistently at the present time.

Sir William Preece. Sir WILLIAM PREECE: I feel somewhat in the same position as the blank leaf between the Old and the New Testament. I am here as a past

President of the British Institution of Electrical Engineers, but I am also an Honorary Member of the Institute of Electrical Engineers of America. I therefore speak in the blank-page capacity. I also hold the position of never having compromised myself by associating myself either with the direct current or with the alternating current system. I have used both when I thought either was right. I believe there are circumstances in which the one is essential, and under different circumstances, where the other properly comes in. Sir William Preece.

Now, we want to consider the subject before us from different points of view. We have to consider generation, we have to consider the distribution of our currents, and we have to consider the transmission of these currents to great distances. In the first place we have to deal with the generation of our currents, and there we start exactly from the same point, for we must all remember that an ordinary dynamo is nothing more nor less than a simple alternator. So we start from the same basis.

Mr. Ferranti commenced his remarks by referring to some of the disturbances. He dealt with the electrolytic effect upon pipes and metallic coatings. There is another serious disturbance that has caused many of us anxious moments, and that is the disturbance produced by alternating currents upon telephones. I have always said that the telephone is perfectly competent to take care of itself. The alternating current engineer need not worry himself about telephones. The telephone is never complete until it has worked on metallic circuit, and when that metallic circuit is tested and properly maintained, no alternating currents, whatever their frequency, whatever their strength may be, can possibly affect telephones. There is another serious difficulty that we have met with in our practice, and that is disturbances to railway signals. Although under ordinary normal conditions alternating currents cannot affect the railway signals, there may be sudden rushes to earth due to those strange effects that were once called, and rightly called, Ferranti effects, certain sudden effects of momentum in alternating circuits which raise the voltage so that the insulation is pierced by great surgings of current. Such things have produced, and will produce, false signals on our block system unless some step is taken to prevent them.

The rest of the disturbances are comparatively trifling in their frequency; but they have been serious in their consequences. There are the fires that have been caused by some of these surging effects to which I have alluded. Now, in determining the necessity for the use of either system, we have not only to consider what I have said, but we have to consider the use to which it is placed. There is first electric lighting, next motive power, thirdly traction, and fourthly the transmission of power to a distance. I do not think there is a single man in this room who would not agree that the only practical and the only possible way to transmit energy to a great distance is now the triphase alternating current system. No high pressure continuous machine has yet been constructed which could possibly do what is being done between Niagara and Buffalo and in California and in Switzerland. There can be no question that for transmission the alternating currents must be considered pre-eminent.

Sir William Preece.

As regards motive power, we come to quite another question. There is no doubt at the present moment that the triphase motor is as good as, if not better than, the continuous current motor under some circumstances. Mr. Arnold referred to the fact that in his country the continuous current was holding its own and displacing the alternating current. Here in France—I forget where at the present moment—there is an illustration, where continuous current machines have been removed, and replaced by triphase, in consequence of the superiority of the triphase over the alternating current. So we have this curious see-saw going on ; at this side of the water the triphase supplanting the continuous current, on the other side the continuous current supplanting the triphase. The moral is, that each in its own sphere is good, and as both are used there can be no serious defects in either the one or the other.

One of the most important questions which has not been raised, and which I should wish to raise, is that relating to the necessity of standardising the frequency. I cannot perhaps remember all the cases, but we commence with Niagara. There the frequency is 25. There is another large installation where it is 45. In many places in England—and Mr. Ferranti himself has started one in the south of London—the frequency is 50. At Deptford, in an installation which he originated, the number is now 67. We come to the City of London, where I think it is 97 or probably 100. In our first alternating current systems in London we started at Sardinia Street with 130. Now we find that the frequency is varying in different parts of the world from 25 to 130, and that shows that there is something wrong somewhere. I believe I am right in saying that the American Engineers would have the standard for motive power 25, for ordinary distribution 50, and for house distribution where each house has its separate transformers, 100. I think that is so, but I have no notes by me to refer to. Anyway, I want to point out that if this joint meeting can possibly do any good it will first allay the impression that there is any difference among engineers as to the relative superiority of alternating and direct current systems; and secondly that now at this joint meeting we might determine a standard frequency that shall apply to various cases.

I have only one more point to make. That is to call attention to one line of progress along which we are working in England. I refer to the distribution of these triphase currents at high pressures to great distances by means of underground cables. Nearly all the experience in America is with overhead wires. The longest underground system which I know is the one established by Mr. Ferranti between Deptford and Trafalgar Square. This is nearly eight miles long. But underground mains to conduct high pressure triphase currents will be laid to a very large extent, and those engineers who will have the designing and manipulation of these must find out and learn all they can of the effects of capacity, for in all I have read and all I have seen that capacity is practically ignored. Everybody considers induction, which is small in its effects compared with capacity. I anticipate, when we get long lines of twenty or thirty miles transmitting triphase currents at

10,000 volts, great difficulties will be experienced in effects due to capacity. Sir William Preece.

Dr. A. E. KENNELLY: The occasion which presents itself to-day for the discussion of this subject is, as we have just heard from Sir William Preece, a most fortunate one, namely, for the expression of opinion as to the best frequency which can be adopted for alternating current systems as well as for the cure or dispersal of some of the mists which still hang over the precincts of the fields of alternating current and direct current supply. I think we are generally agreed, not only in America but also, I think, here and in Europe as a whole, that where you have a densely crowded area to be lit by incandescent lights or arc lights you cannot do better than supply that area with direct current at the pressure at which it is to be operated; and if the area is narrow and constricted you need not exceed the pressure of a single incandescent lamp, or say about 100 volts. But as the area extends, the quantity of copper you put down becomes so large an item of the capitalisation, that you must employ a greater pressure, say 240 or 250 volts, over a distance up to half a mile or a mile. When you come to a greater radius of transmission in your dense area you must employ 500 volts, and finally there arises a limit beyond which it becomes uncommercial to supply the direct current for the direct application and you must introduce a higher pressure. To introduce this higher pressure you must resort to alternating currents, and so in many of the large cities of America you will find that the heart of the city is supplied by a direct-current system on the three-wire plan, and then the outlying regions of that same system are supplied through the medium of alternating currents at higher pressures. The pressure increases as the distance of transmission increases. We are all agreed that when the distance of transmission is considerable you must employ the alternating current. On that ground we all stand. Thus when you have to supply electric traction systems at a distance, you inevitably employ the alternating current, and then you resort at the distant end, at least in America, to the direct-current supply through the medium of converters. The connection together of those two systems. is accomplished with difficulty as well as at considerable expense. There is a set of transformers to reduce the pressure, and then there is rotating machinery required to supply the direct current. This condition of affairs is so anomalous and inconsistent with the otherwise great simplicity of electric current supply, that it is difficult to understand how so remarkable a combination arose. I think it may be claimed to be due to the fact that the electric motor for tramways came into existence gradually and developed as part of a direct-current system, and that up to the present time the standard traction systems of America have been all direct-current systems. The difficulty of supplying all the apparatus needed upon an alternating-current basis, even supposing there were no objections and difficulties to be met with in the alternating-current motor, have constituted reasons sufficient to account for the anomalous condition of affairs. If, however, in the future the difficulties which remain in the way of introducing induction-motor street-cars can be cleared away, we may expect, as I think Mr. Arnold intimated, that this condition will be Dr. Kennelly.

Dr. Kennelly.

eliminated, and we shall have nothing more than a plain alternating-current system from beginning to end. The only considerable disadvantage would lie in the difficulty in maintaining a steady distribution of pressure. The difficulty which is encountered in the matter of the distribution of direct currents on electric railroad systems supplied from a distance is, as we know, the electrolytic difficulty. There has been much trouble in America on this account. Many pipes have been destroyed and others have been much damaged by this means. But that difficulty is giving way to careful and deliberate engineering, and it is a much less serious difficulty at the present time than in the past, because engineers know better now what to do. The damage has occurred not so much to large mains as to service pipes crossing the streets, and also to telephone cables where the metallic sheathing is continuous. By careful attention to these conditions, by studying the outlines of the system carefully, this electrolytic difficulty has been, and can be in the future, largely eliminated in maintaining a difference of potential between pipes and tracks not exceeding one volt within the danger area, by putting down a sufficient amount of ground-return copper, and by carefully bonding the tracks. This danger from the electrolytic action resulting in corrosion can be largely eliminated, and we may expect by engineering skill that this trouble will be almost entirely overcome.

One disadvantage, however, which has not been pointed out by preceding speakers in the direction of alternating current traction consists, I think, in the increased hazard from shock and increased danger to life and person. An accidental shock from 500 volts of direct current is not, as a rule, a serious thing. There are cases on record, I believe, where it has proved fatal, but they are certainly very rare, and the number of shocks which are accidentally encountered from day to day over a large number of traction systems is considerable. If, on the other hand, you employ 500 volts of alternating current and get an accidental shock from that, the shock may be much more severe because it would seem from recent experimental researches that the danger from shock is the danger of disorganising the action of the heart. The danger in a shock from the direct current is in the first impulse, and may be recovered from; whereas with the alternating current you get a succession of shocks which may be sufficient to disorganise the heart beyond all chance of restoration.

There are, of course, other outstanding difficulties with alternating current transmission on railroads, but they are all seemingly of minor consequence. There is, for instance, the disturbance affecting the magnetic needle in general and those of a magnetic observatory in particular. But it seems to me that the worst that can happen in that case is the banishment of the magnetic observatory from the vicinity of civilised communities to the more desert regions of the earth. While that may be a misfortune and an expense in particular cases, yet the aggregate advantage to the entire community of giving a traction system on the one hand, and removing an observatory on the other, does not appear to be worthy of comparison.

Professor Ayrton.

Professor W. E. AYRTON: The special point to be decided at this discussion, as Mr. Ferranti brought out, is the relative advantage of the alternating and continous current, especially with regard to interference with other interests. Interference is the main question we have before us in accordance with the title. There is no question whatever that we have had in various countries a very large amount of interference, and therefore I need not go into the details. Electrolytic interference Dr. Kennelly has referred to, as well as others, also telephone interference. A very serious kind of interference has grown up within the last two or three years, namely, interference with submarine cables. A very serious case happened in the Cape of Good Hope, where a large amount of damage was done, and there was considerable stoppage of messages coming by the Western cable to Europe. There has been interference with the magnetic observatories that Dr. Kennelly mentioned at the end of his remarks. There is no question about these interferences, but there is the question how are we deal with them? Should you endeavour to destroy the attack, or should you allow the attack to remain and endeavour to improve the defence? Those are two totally different methods of dealing with the subject. In other words, should you endeavour to construct each undertaking in such a way that it will not cause any interference with anybody else, or will you start with assuming that there is war? Are we to assume that the other side are sure to attack us and that they will not mind our losses and griefs, and all that we can do is to try to defend ourselves? To a certain extent both practices, both plans, have been followed, in Great Britain at any rate. The Board of Trade regulations state that there shall not be more than seven volts difference of potential between any two points of the rails if the rails of the tramway are used as a return. That is an indication of an endeavour to prevent the enemy attacking us too much, allowing them to fire at us but not with expanding bullets, so to say. The regulation does not say how long the line may be with such a restriction, whether it is to be a long one or a short one. Still the seven-volt rule applies. Now, it has been shown conclusively that the seven-volt rule does not give sufficient protection. It certainly does not give sufficient protection in the case of a submarine cable which lands anywhere near the place where the tramway runs. It does not give sufficient protection in many cases as regards electrolysis, and obviously it would not give sufficient protection in any magnetic observatory which might be located in the neighbourhood of the tramway. In the case of telephones it is possible to obtain a very good defence, and to make the telephone people more or less independent of attack. It was indeed the ease with which that defence can be constructed that led the Joint-Committee of the House of Commons and the House of Lords, before whom this matter was brought a few years ago, not to interpose restrictions in the construction of tramways, because the conclusion they came to from the evidence they heard was that a telephone system in Great Britain was so shockingly bad in consequence of the use of the earth as a return—there was in fact so much interference of one line with another—that even if there were no electric tramways at all, and no distribution of electrical energy on a large scale, it would be neces-

Professor Ayrton.

sary for the telephone companies to resort to modifications. They would have to do that apart from electrical distribution, and therefore it was not necessary to restrict electrical distribution because telephones had to adopt their own defence. But regarding the water pipes and gas pipes that does not apply at all. They cannot use anything in the nature of an insulated "return" which will prevent electrolysis.

Now I come to a very important point which has not been suggested by any of the speakers so far—though perhaps I am wrong in saying it was not suggested, as it was implied by Mr. Ferranti—viz., that it would be very different if direct currents were used instead of alternating. But any doubt of the truth of that assertion has not been suggested by any of the speakers. If we were to adopt universally a general system of alternating currents, should we be free from electrolysis? This is a point which has been interesting me for some time past, and I was making some obseıvations on that subject this year in Geneva, where they have suffered a great deal from damage in the pipes. The matter was referred to me to investigate, and I will only remark that it is not at all clear that if only alternating currents were employed the pipes would be free from electrolysis. Some years ago I carried out experiments and showed an interesting one to certain people in my laboratory. I took an ordinary sulphuric acid voltameter, the Hoffmann voltameter, with two tubes and a platinum electrode in each tube. The hydrogen was evolved in one tube and the oxygen along the other. We sent an alternating current through, and the same thing occurred as with a direct current, namely, the hydrogen came off in one tube twice as fast as the oxygen in the other. It is clear that we cannot assume that there will be no electrolysis because an alternating current is employed, and I find on this table a sample, of which I did not know previously, which has been sent by Mr. Trotter, of the Board of Trade, illustrating that very thing. The label says, "One of a pair of lead pipes buried in a box of earth corroded by one ampere alternating current passing from pipe to pipe for six weeks." That is the result. That is not unlike the corroding seen in Geneva during my investigations. Mr. Trotter mentions in his letter that the specimen was corroded by an alternating current coming from Deptford. That does not mean that the Deptford current is worse than any other. He states that the current came from Deptford to show that it was not any fancy laboratory current. I admit mine was a laboratory current, but it was produced at any rate by a Ferranti dynamo. The letter proceeds "to show it was no fancy laboratory current—I add it with regret because Mr. Ferranti will argue, I suppose, that less damage is done by alternating than by continuous current. There are no more particulars to give except that the pipes are six inches apart and the sides where the currents passed each other are more corroded than the other parts."

It is clear, then, that alternating current will not give us protection with certainty, and it is clear also that it is a very important question to examine under what circumstances do alternating currents produce electrolysis, and under what circumstances they do not. That is a subject which has attracted a good deal of attention subsequently to my publishing the little experiment I told you about in connection with the

Professor Ayrton.

voltameter, and I shall be very glad to hear from the meeting the results of any experiments which will enable us to settle what should be done with a lead pipe so as to make it, if possible, immune to-action by an alternating electric current. For it is not possible to do that if such action occurs. If I polarize my platinum plates by means of a direct current first, then decomposition by means of alternating current takes place with perfect ease. I do not mean to say that the same amount of gas came off for a given number of coulombs which would have come off with the same number of coulombs of direct current, but I mean that when the observer saw the voltameter without looking at the ammeter at all, he saw no difference whatever between the hydrogen and the oxygen coming off with the alternating current as compared with that from the two separate tubes with the direct current.

Speaking now about the defence, about arranging our pipes (water and gas), and the submarine cables, so as to prevent the attack hurting us. In spite of what Dr. Kennelly said, I think more might be done to avoid the attack, and I am happy to say the London Tramways Companies have not looked at the matter at all from the drastic point of view that Dr. Kennelly has just suggested. The London Tramway Companies have *not* said, "We are coming. You have magnetic observatories near London. If we destroy them we are very sorry, and you had better go somewhere else." They have not taken that line at all. The line they have taken is, "Will you try and find out for us what we must do, the least we must do, to ensure you immunity from disturbance?" The result, as some of you know, has been that a Joint Committee was appointed by the Board of Trade to carry out experiments and find out what was the magnetic disturbance produced by existing electric railways and tramways in Great Britain, when those tramways or railways worked under the Board of Trade regulations. When the difference of potential between the rails did not exceed seven volts, what was the disturbance? We found the magnetic disturbance very considerable and the seven-volts limit was quite impossible in the neighbourhood of the observatory.

Without giving you a long account of all the experiments, I will give you practically the final result. The final result of the experiments and negotiations, for which we have very much to thank our British President and Professor Rücker, for they have taken a very active part in connection with these negotiations, has been this: Within two miles of the observatory the Tramway Companies offer to cut up their line into one-mile sections—that is to say, no part of the line in that neighbourhood shall be electrically continuous for more than a mile, each mile being insulated electrically from the rest of the tramway system, and that current shall be brought to the trolley wire at the middle, and taken away from the rails at the middle of each mile section. Further, that no point whatever of the rails of any of these sections shall be allowed to differ from the potential of the earth by more than *one-fifth of a volt*. We have assured ourselves by calculation and by experiment carried out in different parts of London and Great Britain, that with such a difference of potential magnetic instruments will pro-

Professor Ayrton.

bably not be disturbed to an amount that will be practically serious; that is to say, by an amount that will interfere with the ordinary observations carried out in a good magnetic laboratory. So I am very happy to say that the tramways, although they are about to use American plant, have looked at the matter from what I may call in this case a *non*-American point of view. And instead of relying on a supposed superior importance possessed by tramways over a study of the earth's magnetism, they have worked cordially with the Government representatives in ascertaining what precautions must be taken to ensure immunity for the London magnetic observatories without introducing too much interference with the commercial working of the tramways.

There is one other point with reference to alternating currents for electric tramways which has not been yet suggested. I was informed that one of the reasons why in the United States alternating currents were not employed was because they found a difficulty in getting a good contact between a trolley pole and the wire when the wire had snow upon it, more difficulty, that is to say, with alternating than with direct current. Since that time, which was in 1897, certain electric railways have been constructed on the Continent of Europe which use alternating current, and it will be interesting if any one here can tell us whether any difficulty has been experienced in the winter when snow rested on the trolley wire; that is to say, more difficulty than would have been experienced had the direct current been employed.

M. Korda.

M. Désiré Korda: Lorsqu'il s'agit d'une distribution dans une grande ville, le courant alternatif, monophasé ou triphasé, grâce à la facilité de sa transformation, offre des avantages sur le courant continu à haute tension qui exige pour sa transformation des transformateurs rotatifs; quant au courant monophasé, la grande objection qui s'oppose souvent à son adoption est la difficulté de son application à la force motrice, le vrai moteur à courant alternatif simple n'étant pas encore inventé. Par suite, dans bien des cas, on préfère adopter le courant polyphasé malgré la difficulté qu'il y a à régler convenablement les trois ponts, et à maintenir les tensions égales; on a quelquefois recours à des systèmes plus ou moins compliqués, polycycliques ou autres.

C'est sur la question des courants polyphasés que je voudrais dire quelques mots, ou plutôt sur un cas dans lequel leurs avantages et leurs inconvénients par rapport au courant continu se montrent d'une façon beaucoup plus évidente, sur le cas de la distribution de force dans une grand usine, comme, par exemple, une sucrerie importante.

Les courants polyphasés, comme vous le savez, messieurs, se sont répandus très rapidement pour la distribution de l'énergie dans de telles usines grâce à la simplicité des réceptrices qui ne nécessitent qu'un entretien presque nul; au contraire, pour les courants continus, le collecteur demande beaucoup d'entretien, une propreté plus grande et plus difficile à obtenir, spécialement dans le cas d'une usine comme la sucrerie dont je parle et dans laquelle on rencontre à chaque instant des liquides plus ou moins visqueux et des poussières qui se fixent près du collecteur.

Assez souvent, j'ai été amené à comparer ces deux systèmes dans

des projets et j'ai eu là l'occasion de voir leurs avantages et leurs inconvénients respectifs. M. Korda.

Les quelques points sur lesquels je me permets d'attirer votre attention sont les suivants.

Au point de vue de l'entretien, comme je viens de le dire, et pour le cas que j'ai cité, il n'y a pas à hésiter, tout l'avantage est pour le courant polyphasé.

Seulement, voici un inconvénient qui se présente. Dans une usine comme celle que j'ai prise pour exemple, il est impossible d'adopter des courants à haute tension, précisément à cause de ces liquides visqueux et de ces poussières dont j'ai parlé ; la propreté sur laquelle on peut compter est trés relative, et l'encrassement qui en résulte interdit l'emploi de ces courants à haute tension. Aussi la direction de l'usine limite l'ingénieur électricien à une tension qui ne dépasse guère 200 volts, parceque les tensions efficaces dépassant 200 volts pour un courant alternatif donnent comme valeur maxima des tensions qui commencent déjà à être considérées comme dangereuses. Or, dans cette usine, vous avez à actionner de nombreux moteurs, des centrifuges à sucre, par exemple de grandes dimensions, et ces centrifuges demandent du courant de grande intensité, surtout au démarrage.

Il faut donc canaliser des courants puissants. Or, le propriétaire de l'usine veut dépenser le moins possible pour la canalisation, et, à cause du prix, préfere les cables nus aux cables isolés. Comme on ne peut pas tordre ensemble les trois cables nus comme on aurait pu le faire avec les cables isolés afin d'éviter l'effet de la self induction, on est obligé de les placer aussi près que possible les uns des autres. Mais il y a forcément une limite à leur écartement à cause de la tension et de l'état de propreté peu sur. Voilà donc encore une nouvelle cause de courant dewatté pour l'ingénieur électricien chargé du projet d'installation, surtout si la station centrale d'énergie électrique se trouve un peu éloignée des ateliers dans lesquels la force motrice est employée. Si elle est, par exemple, à une distance de 200 ou de 250 mètres, la self-induction de la ligne va jouer déjà un grand rôle dans une usine d'une certaine importance.

A l'augmentation de l'intensité du courant provenant du décalage des moteurs vient donc s'ajouter la chûte de tension provenant de la self induction de la ligne.

En outre, dans bien des cas, l'on ne vous accorde pas une machine motrice spéciale pour l'excitatrice ; le propriétaire de l'usine se dit que, s'il employait du courant continu, la même machine lui donnerait l'excitation, et qu'il n'aurait pas besoin d'une machine spéciale. Se basant sur ces considérations, il vous demande donc d'établir un devis en prévoyant l'excitatrice sur la même machine que la génératrice. Il en résulte une troisième raison qui vous limite en ce qui concerne le voltage du réseau. En effet, au moment de la plus forte charge de l'alternateur, la machine à vapeur se ralentit ; l'excitatrice se ralentit aussi, et, quand l'on a le plus besoin de voltage, l'excitatrice, comme l'on dit vulgairement, nous laisse en plan. Cette diminution du voltage est, en effet, comme vous le voyez, en raison directe du carré de la diminution du nombre de tours de la machine.

M. Korda.

Voilà donc trois raisons qui font que, pour des applications du genre de celle dont j'ai parlé, on est forcé de choisir une machine d'une capacité plus grande que si l'on employait du courant continu.

Voici maintenant que le remède commence à se faire jour. Comme vous le savez, l'on a commencé, dans ces derniers temps,—et c'est l'Exposition Universelle de 1900 qui en a fait la première application pratique—à s'occuper du compoundage des alternateurs; je crois, en effet, que, dans des cas analogues, le compoundage est tout indiqué, et non seulement le compoundage mais l'hyper-compoundage, car, non seulement il faudrait pouvoir compounder les alternateurs, mais il faudrait aussi pouvoir arriver à l'hyper-compoundage en ce qui concerne la canalisation pour tenir tète à l'abaissement du voltage provenant de la self induction de la canalisation et des moteurs.

Je crois, par conséquent que, le jour où cette application dont nous avons vu la première apparition pratique à l'Exposition Universelle se généralisera, la situation du courant polyphasé, par rapport au courant continu s'améliorera encore.

Tout ce que je viens de dire n'a rien à voir avec une distribution d'énergie électrique dans une grande ville, parceque dans ce cas, l'on a une haute tension, et la chute du potentiel est proportionnellement très faible; les intensités étant très faibles, la valeur de la contre force électromotrice de self-induction correspondante est faible: aussi ces difficultés dont je parlais s'éliminent en quelque sorte d'elles mêmes. Mais lorsque l'on a une grande usine à desservir, et qu'il faut employer de grandes intensités de courant sous une basse tension les trois causes que j'ai citées se présentent, et l'on est obligé, naturellement, d'en tenir compte.

Professor Crocker.

Professor F. B. CROCKER: I think now that the discussion has been opened, and the subject generally covered, that the time has come when we may confine ourselves to a few special points. The subject of our discussion is with special regard to interference. I do not think that is the most important point, but as that is the subject we should apply our remarks largely to that. There are two interference effects that electric currents produce. I think they might be classified as the inductive and the leakage effects. Under the head of inductive I should include the magnetic effect, because the inductive effect is the magnetic effect, and *vice versâ*. Now, the inductive effect of the direct current is purely magnetic, it produces only magnetic effects in its vicinity. Therefore its disturbing effect is upon magnetic apparatus in magnetic observatories or upon magnetic apparatus generally. But there are not many magnetic observatories in the world, and, as Dr. Kennelly observed, they might be relegated to a point where they would not be interfered with. It does not seem to me that the progress of the electric art ought to be influenced greatly by the existence of a few magnetic observatories, and certainly it would not be so in many places. So I should be inclined to agree with my fellow-member and dismiss that point as not very serious, to say the least.

The alternating current also has an inductive effect, viz., the production of currents in its neighbourhood. It also produces a field, but that field being alternating produces no permanent effect upon magnetic

Professor Crocker.

apparatus. But the current inductive effect is produced solely by the alternating current, and that I should consider the more serious, because we have many more telephones or even telephone exchanges than we have magnetic observatories. As Sir William Preece has said, we can largely avoid that inductive effect by metallic circuits. But there are cases where we want wires overhead, and under those conditions we can hardly eliminate the inductive effect except by transposition of the wires, which is not a complete preventive. Furthermore, in certain cases and in smaller towns grounded circuits are used, and it seems to me that the inductive effect which produces a current in a neighbouring conductor is a more serious disturbance than that of the production of a magnetic field. I think that is so at the present time, and I think it will always be so. Therefore I should say that on that account the alternating current is more guilty than the direct; in other words its disturbing effect without leakage is greater than that of the direct current. Now, the leakage effect, unfortunately, is much more serious with the direct current. The leakage of direct current is that which produces the electrolytic effect, and is the most serious interference that electric currents produce on other apparatus or other interests. But that leakage is something we can control to a great extent. The production of a magnetic field and the inductive effects are much more elusive than mere leakage. In high-tension conductors, overhead or underground, the leakage quantity is exceedingly small. It must be so. If considerable leakage occurred in an underground conductor at several thousand volts it would produce a short circuit or a ground. But there is a leakage occurring in low-tension conductors. I think that is because we have allowed it to exist. If we insulate high-pressure conductors as well as we do, we can insulate low-pressure conductors equally well or better. I had occasion to test the underground network of New York City, many miles in extent, and the system was split up in sections to enable this to be carried out. We found that the leakage was not excessive; it is a very small percentage of the current—considerably less than 1 per cent., as the test showed at the time. I am referring to electric lighting conductors which are not grounded. A similar result is found, I believe, by a comparison of the total output with the output that is useful. That shows that the leakage is a small quantity. In the gas industry that is not the case. A very large percentage of gas—10 or 20 per cent. in the case of New York—is lost by leakage. But that is not true in electric distribution. So I should say that the fact that we have now in electric conductors, overhead or underground, a considerable leakage, is a temporary condition which can be overcome. I am sure electric conductors, except those purposely grounded, can be so laid that the leakage is a negligible quantity, even for long-continued electrolytic effects. Now, with the grounded trolley system, the single-wire trolley, the current must go into the earth. But there again by the use of improved methods, return feeders, and more perfect bonding, we have reduced that promiscuous flow of current through the earth, until now it is very much less than it was before, and I think it can be brought down to a quantity which is also insignificant. If it can be reduced to the figure which Professor Ayrton

Professor Crocker.

mentioned—one-fifth of a volt—if that were the difference of potential, it would be far below any dangerous electrolytic limit. So, apparently, the interference is not so very different in the two cases—that is to say, the alternating has a greater inductive effect without actual transfer of current, and the direct current has much more serious electrolytic effects. But those can be and have been largely overcome by more perfect construction.

I should like to say just a word on the motor question. Sir William Preece cited a certain instance where direct current machines had been replaced by alternating. I know of several other instances where alternating have been replaced by direct, so that evidence is not at all conclusive. That point alone could well occupy us for much more than the time we have at our disposal. I will simply say now that so far as efficiency is concerned the two kinds of motors are almost identical. I have in my possession efficiency curves of the latest induction motors from the Westinghouse and General Electric Companies, and I compared them with the efficiency curves of direct current machines of the same size and the agreement was almost perfect; the two curves coincided almost exactly at full load and at all loads above one-third of full load. Below that the agreement was not so close.

A Member.

A MEMBER: What do you call the same size?

Professor Crocker.

Professor CROCKER: I mean in rated capacity, also in actual capacity. The agreement of the two sets of curves was remarkable and complete, except at very small fractions of full power the direct-current motor has a better efficiency, but at one-half to full load the agreement was almost perfect.

Sir W. Preece

Sir WILLIAM PREECE: What was the efficiency in per cent.?

Professor Crocker.

Professor CROCKER: It depends on the size. The efficiency depends on the capacity of the machines. For example, a small machine, a one kilowatt alternating-current induction motor, would have the same efficiency as a one kilowatt direct-current machine between half and full power, and the curves representing that efficiency would agree exactly.

Sir W. Preece

Sir WILLIAM PREECE: What percentage of 100?

Prof Crocker

Professor CROCKER: That depends on the size of the machine.

Sir W. Preece

Sir WILLIAM PREECE: Take one kilowatt?

Prof Crocker

Professor CROCKER: I do not recollect that figure.

Sir W. Preece

Sir WILLIAM PREECE: Was it over 90 per cent?

Prof Crocker

Professor CROCKER: No, it was not; but we compare the two systems, and I say there is no choice in that respect, except that the direct-current motor was higher efficiency at light load; at other loads the two are equal.

Now as to regulation for constant speed, the two machines are equivalent. A percentage of reduction of speed occurs when the machine is loaded from zero to full load. First-class direct and alternating current machines are equally good in that respect for constant speed; but when you regulate for variable speed the direct-current motor has a great advantage over the alternating-current motor. It is equal in efficiency, it is equal in regulation for constant

speed, but the direct-current machine is superior for variable speed. That applies not only to stationary motors, with which I am most familiar, but it applies also to electric railway machines, and I think is equally important for stationary and for traction purposes. It is in that respect that the direct current has its advantage for power purposes over the alternating. I do not think it would be fair to the direct current, or proper to this occasion to allow that point to pass unquestioned. Professor Crocker.

Mr. W. M. MORDEY: With several of our American visitors I have discussed during our meetings the question of earth drop on the return circuits of tramways, and I have found a general disposition to suppose the Board of Trade regulations restricting the drop within 7 volts was not adhered to, and could not be adhered to. I have just carried out a very complete series of tests on many miles of tramways in England; and one of the things I had to investigate was that question of earth drop. I found in that system, running under the fullest load conditions, that the drop of potential never exceeded 5 volts. You will notice in tramway stations that the recording instruments which we always use, but which I think are not used in America, may momentarily indicate more than 7 volts; those instruments indicate higher momentary effects than the real value. For instance, if you suddenly switch one of those instruments on to a steady pressure of 6 volts, it will usually swing to about 10 volts. One of the differences between English and American practice is, I think, apart from the question of scale, that great attention has been given to this question of earth drop, and to the continuous recording of what is happening. The Board of Trade regulations are strictly enforced. I am sure the result has been good. I hope in this discussion we shall obtain from our American visitors some actual quantitative results arising from electrolysis. The real point which I think was intended to be brought out was whether or not electrolysis, quite apart from other questions, would not be a determining factor in the development of systems of supply, at least where earthed conductors were used. Mr. Mordey

I will ask you to bear in mind the one and only absolute cure for all troubles of this sort, whether direct or alternating currents are used—the insulation of both conductors. I expect to find it applied in the future in all railway cases and in all conduit cases, whether for direct or for alternating current. Quite apart from the avoidance of electrolytic effects it has many advantages. To comply with Board of Trade regulations we have to use boosters and great quantities of copper to assist the return circuit. There would be no objection to 25 or 50 volts—or even more, instead of 7—if we had not to consider electrolysis. We could, if we wished—and if it were economical to do so—have as big a drop on our return as on our trolley line if we used an insulated return; and we should get rid of bonding and of sparking at dirty rails, and of electrolysis. Information as to the results of the working of the double trolley system at Cincinnati, where it is or has been employed on a large scale, would be of great assistance to us. We should like to know if, in America, in your conduit or railway work, you are making any efforts to avoid the objectionable earth return.

Whether in the future there will be any preponderating system

Mr. Mordey. still seems quite uncertain. It may be the system is going to be alternating for transmission and direct for distribution; but I cannot help thinking that ultimate simplicity will lead to the use of alternating currents for almost everything, at least where we have long-distance transmission or large areas. In railway work where we begin with alternating currents, the simplicity of transformation will probably ultimately lead to the use of alternating throughout. There is, however, much to be said for Mr. Lennard's system, with his rotary convertor on the car, if direct currents have any part. My own feeling, in spite of present fashions in England, is that sooner or later we shall have in all large systems the alternating current right through. I believe the rotary transformer is a make-shift, to be cleared away sooner or later for railway work; it is quite unnecessary for lighting and only indispensable for electrolytic work.

Some of you may go on to Switzerland from here, and may see the examples of alternating work there. You will see the Burgdorf-Thun Railway and the Engelberg and Jungfrau Mountain Railways, where many questions are disposed of. If you can start a train on a mountain rack railway you can start one anywhere.

We have, during the last few days, seen the Central London Railway, and highly appreciate the facilities afforded us by the Company during our visit. But all of us who have had to do with the estimating of engineering work will have felt that if all the cost of those sub-stations—their static and rotary transformers and so on, their first cost, their maintenance, their working cost in power and labour—could be realised and put into conductors, we should have, for that case at least, a simple system which would be safer and probably more economical in first cost, certainly in working cost afterwards.

The question of capacity has been referred to. It is more serious in alternating underground work than is generally realised. I recently had to investigate some difficulties due to capacity on a system of two hundred and fifty miles of underground cables in St. Petersburg. There is really much less difficulty than is generally supposed in overcoming most of the effects of capacity of such mains. I wish I had time to say more on the subject, but must leave it to an opportunity which I hope may be granted later.

I should like to refer to one "existing interest." I mean the interest of the public and of the scientific laboratories and observatories. There can be no question that ultimately these institutions will have to be removed from the centres of large populations where there are great applications of electricity, unless the methods adopted in those institutions can be so ordered as not to be interfered with by such applications. Fortunately we have at the Board of Trade a striking object-lesson. As far as I know, the Board of Trade electrical standards laboratory is the only electrical institution in England which is legally obliged to be accurate. It is laid out in such a way, the ponsible officials say, that although they are in the heart of London do not mind what electrical applications are made in London; rrangements are such that they can carry on their work with the cy demanded by law without any reference to what is going on

outside. That does not touch the question of measuring the magnetism of the earth. But surely that should be measured where it is the magnetism of the earth and not that due to the application of electricity to the use and service of man. There was a case in London of an institution, whose work I would not for a moment depreciate, where the influence of that institution was exerted successfully to prevent what would have been a very great convenience to the population of London, the running of an underground electric railway. It is difficult to speak calmly of such an action. There was no sort of proportion between the two interests; yet, though the object of the scheme was so excellent, the smaller interests were allowed to stand in the way of the benefit and the convenience of the population of London. Mr. Mordey

Mr. C. O. MAILLOUX: The subject has been so well discussed it has been nearly exhausted already—that there is very little to be added. There is one point only which I think has not yet been fully discussed. In our country we would take a broader interpretation of the topic of discussion than is being taken here. The effects produced upon certain industries oftentimes effect the feasibility of the industry, and in this connection I have been able to note the practical difference between the two systems, and the one which in fact determines the feasibility of either one, or its want of adaptability, according to the case. One of the important points, which it seems to me has been neglected, is the influence of the power factor in the case of the alternating current. My colleague, Professor Crocker, has pointed out quite clearly the similarity in results obtainable between the alternating and the direct-current motors. He has also pointed out that at constant speed they work quite alike. But he has neglected to note that the influence of the power factor on alternating currents is a matter of importance. As we know, when the electromotive force of the source of supply of a direct-current shunt-wound motor varies, the magnetic field of the motor varies, but that has the effect of merely changing the armature current to an extent necessary to maintain the speed; so that although there is speed fluctuation, it is not so great as when the electromotive force varies in the case of the alternating-current motor, because the speed is in this case a higher function of the electromotive force. In our country we have several isolated plants which assume the dignity and importance of central stations. It is not uncommon for us to have isolated plants of one thousand to three thousand horse power, which distribute energy over a zone of about half a mile radius. It is under such conditions that these difficulties are to be noticed. I remember an instance where a certain process for manufacturing coffee was employed, the electric current for which was furnished from a power station which also supplied lights to the district. It was found that owing to the enormous wattless current produced at the starting of the motors, on many occasions the line and even the generators would be overloaded, the result being that the current supply was cut off by the fuses blowing off, and that the machinery which was used in that coffee mill was put out of action, oftentimes at critical moments, when the stoppage meant great loss. The alternating-current motor system became absolutely inadmissible in this case from that circumstance, and Mr. Mailloux

Mr. Mailloux.

had to be abandoned. One of the great objections, as you know, to the alternating-current motor of to-day is the fact that it takes such a large current at the time of starting. Its large wattless component is a serious matter, affecting as it does the working capacity of a line where the motors are constantly stopping and starting. Where all the motors are running together at an approximately constant load and at constant speed, the problem is simple, and there are no industrial reasons why the alternating current should not be placed upon an equal footing with the other. But where the motors are constantly stopping and starting, and especially where the distribution of power is constantly changing, and also where you load certain feeders more than others, the disturbance becomes very important. It has to be seriously taken into consideration by the designing engineer in laying out the plant. I have had occasion to instal a plant in a sugar refinery which required 2,000 H.P., and it was decided to adhere to continuous-current motors partly for these reasons, and also for the reason that in cases where variable speeds are required, and where the machinery is required to run at intermediate speeds, there can be no question that the direct-current system is at the present time the most suitable, if not indeed the only feasible one. I wish in this connection to speak of the pertinent remarks made by our French colleague in reference to one of the features, one of the industrial conditions, which oftentimes influences the selection of systems. I refer to the cost of the wiring. It is true that to-day, at least for the larger distributing conductors in buildings, and within a short radius, as well as for underground purposes (where we are obliged to use concentric or twisted conductors), the cost of the wiring is necessarily much greater than in the smaller branches and net works, such as the local lines or mains which run to individual motors. In our country the problem is, to some extent, solved, since we use iron conduits any way for these lines. Our insurance regulations prescribe, indeed practically compel, the use of iron conduits for the smaller lines, and, usually, we have two conductors, twisted or concentric, as the case may be, laid in iron conduit, whether the circuit work be used for direct or for alternating currents; but in the case of the larger conductors the arrangement need not necessarily be the same in both cases. With alternating current we must still use a twin or duplex conductor laid in iron conduit, to keep down the reactance drop. With direct current there is no reactance, and we are not compelled to use twin conductors. It is often found to be very much cheaper to use two separate conduits, and two ready-made single conductors (especially in the case of the larger feeders or mains), than to use one single conduit very much larger in size and a cable specially made at relatively great expense. The mechanical difficulties in laying the larger conduits, their greater obtrusiveness and the greater space occupied by them, especially at bends, turns, offsets, &c., would be sufficiently objectionable even if they did not, as they do, in most cases, make the cost relatively greater.

Professor Thompson

Professor SILVANUS THOMPSON: We have had no observations wh[illegible] the relative interference of alternating and continuous c[illegible] ship. It is of absolutely vital importance that there

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shall be no interference with the magnetism of the compass, and yet, extraordinary as it may seem, almost all ships that are equipped with the electric light are equipped with continuous current apparatus. I never could understand why that was. Some of the earliest vessels had alternating currents on a three-wire system, with the skin of the ship serving as a middle wire, but this system was succeeded by one with the very worst kind of continuous-current generator which could be put on board, viz., the bi-polar. I hope to see a complete revulsion in ship-fitting from this plan.

No reference has been made to the somewhat greater danger of fire that exists where conductors are carried through damp places if those conductors are served with continuous current. Electrolysis beginning at some leak will develop a current which eventually heats and destroys the insulation at that point, and thus originates a fire. With an alternating current you are less likely to have that occurrence. On the other hand, I suppose switch-makers will tell us that alternating switches are more expensive than continuous. No one has mentioned that while for arc-lighting admittedly there is some advantage in using continuous currents, for glow-lamps there is an advantage in using the alternating current. Not that there is any higher efficiency: that fallacy has long been disposed of; but if you use a continuous current for glow-lamp purposes, and the distribution is to take place over any large area, it becomes absolutely necessary to go to the high-voltage lamps working 200 to 250 volts. Now any one who has taken the trouble to measure the reputed efficiency of high-voltage glow-lamps will know how inferior they are to the 100-volt glow-lamps; things which are supposed to be taking 3½ watts per candle being found actually to take 6, 7, and 8 watts per candle. I think there will be a great revulsion when the facts are known about the inefficiency of high-voltage glow-lamps. I prefer to have 50-volt glow-lamps; they are better in every way, and last longer. This is impracticable with a continuous current, but with the alternating current it can be done where there are house-to-house transformers; and so, using low voltage lamps, you can work to a much greater distance with alternating than with a continuous current.

I was sorry to hear Dr. Kennelly rake up the fallacy of there being a greater danger to persons from the alternating than from the continuous current. But that is a revival of a bit of the old electro-politics. When people wanted to damn the prospects of alternating currents and show how much better they were for electrocution, this was the line they took. I hope those arguments have disappeared. I think there is some evidence that the alternating current is not so dangerous as the continuous current; that the shock which is given by the alternating currents throws backwards the unfortunate person who receives it, and does not contract his muscles upon the conductor in the way that a continuous current does. The researches of Prof. H. F. Weber must not be overlooked. I prefer to leave that question to M. d'Arsonval, who understands these electro-physiological effects better than we electrical engineers.

Professor Ayrton raised the question of the electrolysis produced

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by the alternating currents. Might I point out that the question whether alternating current will, in any given circumstances, produce continuous electrolysis depends very largely upon the question of the relative area of the electrodes employed and the density of the current. Because whether gas is disengaged at that area or not during the period depends on the density to which that gas accumulates, and whether it is given off. In fact the question whether the polarisation becomes irreversible or not is very largely a question of scale.

Lastly, I will draw attention to certain points connected with electric traction on a large scale. I re-echo the suggestion of Mr. Mordey, that it is well worth while for one who has not seen those electric railways in Switzerland to visit them, and to see how admirably the three-phase current is adapted for starting trains under the most severe circumstances possible. It is known from the experiments of Professor Carus-Wilson that the difficulty of the acceleration of the motor at starting is after all imaginary; and that it requires an extra percentage of current is also a fallacy. Every motor started upon a load takes more current than when running on that load; and this is true also of the tri-phase motor. The current required to produce rapid accleration is even more important than the starting-torque; and the three-phase system, instead of being worsé, is distinctly better.

We have had in London several recent object-lessons on electric traction. One of them has been, from one point of view, a gigantic success, but also a total failure. Little more than a year ago we were told that one of our millionaire railway companies had put down a sum of, I think, £30,000, to have experiments tried upon the Underground Railway. I referred nearly a year ago to this supposed experiment, and pointed out that the one experiment which was wanted for electric traction was to ascertain whether a three-phase motor arrangement would be better than a continuous one. We knew pretty well, but we wanted it tested, we wanted a verification. That experiment has not yet been tried. There were called in several engineers of the highest distinction, and they were aided by the constructional ability of Messrs. Siemens Brothers. But the only thing which has been tried, notwithstanding that all the resources of a great railway company stood behind the experimentation—I say it before Sir William Preece's face—all that they have succeeded in doing is merely carrying out on a large scale what was done at Gross Lichterfelde, by Siemens and Halske, fifteen years ago, viz., establish the fact that you can drive by the continuous current, using conducting rails put beside the ordinary rails. The London press has pronounced this experiment to be a perfect success. I regard it, on the contrary, as an abject failure, inasmuch as it gave us no further information.

Lastly, if you are going to have, as I think we shall have, trains running at 100 or even 150 miles an hour, driven electrically, there will be no chance of success if we have to depend upon a motor which has got a commutator upon it. I speak of a new growing interest, viz., exceedingly rapid transit, and I venture to say that exceedingly rapid transit the only chance of possible success is

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to take advantage of that which is the finest thing of all in electrical engineering, the perfect flexibility and adaptability to requirements afforded by the current when that current is an alternating one.

Mr. Ward Leonard.

Mr. H. Ward Leonard: I have a fair acquaintance with what can be accomplished with the continuous current as regards large starting-torque with a small amount of energy, and have given considerable attention to the operation of large motors which have to be started under very heavy loads, and operated at different speeds and reversed.

I have lately seen one of the recent installations employing three-phase motors upon railway work in Switzerland, and I must say that when the car was started it seemed to me as though there were about one donkey-power doing the accelerating. It did not convey the impression to my mind of being able, with moderate power, to produce the heavy starting-torque required for the rapid acceleration which is such an important factor in electric railway work and many other important applications of electric motors.

Perhaps my views on this point are a little biassed, but it seems to me that when we consider large railway motors the most important points are: rapid acceleration, small starting energy, perfect and simple control and ease of reversing, and the restoration into the circuit of the energy at present wasted upon the brakes.

We find the possibility of obtaining all of these points in the continuous current to a degree to which there is no promise as yet in the alternating current.

When we consider the question of electrolysis, we are met with difficulties in attempting to use a continuous current in the ground return circuit, which seem insuperable except by capital expenditures which are entirely uncommercial.

When we wish to transmit very large amounts of energy over the long distances which are desirable, we all agree that the alternating current is the only suitable one for the purpose.

These considerations have, for many years past, made me believe that we ought to use the alternating current for the generation and transmission of our energy, and that we should have upon the moving vehicle some means of transforming the alternating current into a continuous current of controllable E.M.F., and that we should use this continuous current of variable E.M.F. for operating the propelling motors at the variable speeds required in practice.

These arrangements would give us large starting-torque with a small consumption of energy, and even in the case of the largest motors will give perfect control at any desired speed and simplicity in reversing. It eliminates all difficulties due to the electrolytic action of the continuous current in the ground circuit. It gives us the power to employ motors of practically unlimited power at practically unlimited distances for railway and other variable speed uses, and entirely eliminates the expensive and inefficient sub-stations of to-day.

It seems to me that the electric railway is the application of electric power which is going to exert the determining influence upon the methods of using electric energy in the future, especially the railway motors of very large size; and it seems to me a significant fact that,

Mr. Ward Leonard. after twelve years of development by the leading engineers of all countries, there are no alternating current motors exhibited at this exposition for railway service or any other duty having similar requirements.

Professor Perry. Professor PERRY: Mr. Carl Hering, M. Mascart and I waive our right to say anything on this question, although we could, of course, join heartily on one or other side. I beg to say that we think it better to have no vote upon this discussion as it is an incomplete discussion—one which is adjourned. The specimen sent by the Board of Trade will be on view in our room in the British Pavilion.

At the conclusion of the discussion, brief references to certain objects possessing special or novel interest in the electrical sections of the International Exhibition were made successively by M. Hospitalier, Major-General Webber, Mr. Gavey, and Mr. Hering. Summaries of their remarks are appended, with the exception of those of Mr. Gavey, who has expanded his account of Class 26 into a paper to be read before the Institution in November (see page 73).

NOTICE OF EXHIBITS IN CLASS 23 IN THE PARIS EXHIBITION.

PRODUCTION ET UTILISATION MÉCANIQUES DE L'ÉLECTRICITÉ.

By E. HOSPITALIER.

In the Universal and International Exhibition of 1900, a whole group (Group V.) is devoted to the subject of Electricity; and this group is divided into five classes, numbered 23 to 27, of which the first-named (Class 23) is entitled, *Production et Utilisation Mécaniques de l'Électricité.* In the short space of time at my disposal I can only refer to the machines and apparatus possessing the most novel interest in this class, and I ask your indulgence if my enumeration of these is somewhat suggestive of a catalogue.

Continuous Current Dynamos.—Progress since 1889 in this class of machine is evidenced mainly in the constantly increasing power of the machines. The dynamos exhibited by the firm of Siemens Bros. and Co., of London, and M. Thury's constant-current dynamo shown by the Compagnie de l'Industrie Électrique, of Geneva, merit special examination.

Alternators.—The most powerful alternator shown is that of the Allgemeine Elektricitäts Gesellschaft (3,000 KW.). We should regard as evidence of progress made since 1889 the *amortisseur* of M. Maurice Leblanc (Joseph Farcot) which facilitates the coupling of alternators and their maintenance in synchronism, and the compounding exciters of M. Boucherot (Maison Bréguet) on the one hand, and of M. Maurice Leblanc (Maison Grammont) on the other hand, which permit the maintenance of constant potential, irrespective of variations of the load and of the shifting of phase. The powerful asynchronous three-phase motors of the Westinghouse Electric Co., which operate the moving platform and the electric railway within the Exhibition, are of very great interest and deserve a special visit. In 1889 simple alternating currents, now known as monophase, were alone in evidence, but to-day the greater number of the generating groups and of the machines exhibited are run by polyphase alternating current dynamos. All these generating groups are worth a visit, and to avoid invidious distinctions I prefer not to mention any one of them in particular.

As a step towards determining the limit of pressure that may be obtained direct from alternate-current generators, without using transformers, the Société l'Éclairage Électrique has constructed an alternator generating at 30,000 volts, which is worthy of special attention.

Transformers and Converters.—Transformers for converting alternating into alternating current call for no special remark; but the Société Alsacienne de Constructions Mécaniques show a very interesting booster for three-phase currents.

Motor-generators and Rotatory Converters, which were not in evidence at all in 1889, play a very important part in 1900, and I would call attention specially to the six-phase converter of the Société Alsacienne de Constructions Mécaniques, to the converter shown by the Société d'Électricité Alioth, and to M. Maurice Leblanc's converter with fixed magnetic and electric circuits.

Cables.—Great progress has been made since 1889 in the manufacture of cables, and very remarkable experiments are daily made by the Allgemeine Elektricitäts Gesellschaft, the firm of Pirelli (of Milan), and the Société des Câbles Berthond Borel et Cie., to illustrate the resistance of cables

at high pressure to disruptive discharges, and to demonstrate their electrostatic rigidity.

Accessories.—The handling of currents of great intensity or of high pressure has compelled constructors to study a special apparatus which had no existence in 1889, and of which some idea may be gained by a visit to the two switchboards of the Exhibition for conveying the current from the groups of generators to the mains, the one for continuous, the other for alternating—single-, two-, or three-phase—currents. In the section of foreign generators Messrs. Siemens and Halske exhibit some extremely interesting experiments (in order to demonstrate the efficacy of their horned lightning arresters) in the production of electric flames, nearly two meters in length at the moment of extinction, in which from 300 to 400 KW. of energy are utilised, and producing a temperature sufficient to cause the direct combination of atmospheric oxygen and nitrogen.

Motors.—Tramway and automobile motors show many novelties in comparison with the corresponding class of exhibits in 1889, when the earliest industrial tramway motors timidly made their first appearance. Alternate-current motors, whether simple or polyphase, were then unknown, as well as special devices for starting, of which the Exhibition contains a certain number of interesting examples, among them being those of M. Boucherot, M. Max Déri, and M. Fischer-Hinnen.

Applications.—The mechanical applications of electricity in the Exhibition are innumerable, the machine-tools actuated by electric motors varying infinitely and defying enumeration.

I will now give place to Major-General Webber, who will refer to the exhibits in Class 27, as I have endeavoured to do to those in Class 23.

NOTICE OF EXHIBITS IN CLASS 27 IN THE PARIS EXHIBITION.

APPLICATIONS DIVERSES D'ÉLECTRICITÉ.

By MAJOR-GENERAL C. E. WEBBER, C.B., Past-President, Institution of Electrical Engineers.

This class includes scientific apparatus used in Electrical Laboratory Work; Electrical Measuring Instruments; Instruments and Apparatus used in Electrotherapeutics; the Electric Measuring of Time; Electrical Instruments and Apparatus used in Railways and on Public Works, and various other special applications.

In Class 27 the history of the development of instruments of precision and of measurement is illustrated in the hall of the Musée Centennal d'Électricité, which contains 249 exhibits and 210 books and manuscripts. The earliest of the former is a Coulomb balance, dated 1785, and of the latter a volume on the nature of magnetism, by J. T. Hannonio, dated 1562.

In the memoir on the subject of instruments of precision drawn up by the Commission and published in the French catalogue, we find the following :—

"Les circonstances, en réservant à l'Angleterre les premières opérations de cette délicate initiative, lui auront valu l'honneur d'avoir jeté les bases solides sur lesquelles a été érigé depuis, l'edifice actuel de la science electrique."

The design of those who are responsible for this part of the classification of objects exhibited in Class 27, namely instruments of precision, appears to be one that should be applauded by both engineers and physicists, because it is intended to emphasise the practical spirit of the scientist and the scientific attainments of the engineer, by showing that those instruments which in the hands of the *savant* are used for theoretical discovery, also assist the electrical engineer in his workshop, and in his practical application of the use of electrical energy to the daily use of mankind.

At the head of the list of exhibitors in Class 27, of what may be called instruments of precision, stand Mons. Jules Carpentier, of Paris, and Messrs. Siemens and Halske, of Berlin. These are followed in merit by Ducretet, Gramme, Abdank, and Gaiffe, of Paris; Professor Edelmann, of

Munich; James White, of Glasgow; also Professor R. Arno, of Italy, Hartmann and Braun, of Frankfort-on-Main, and Ganz, of Buda Pest.

It was unfortunate that the merits of the Weston Electrical Instrument Company of New Jersey were not sufficiently displayed by the variety of the objects they exhibited.

The Scientific Instrument Company of Cambridge, of which Professor Darwin is the directing genius, have placed their Callender's Patent Electric Recorder, and their Duddell's oscillograph in Class 15, where their great merits might have escaped attention but for my successful efforts to have them examined by the jury of Class 27.

Although the apparent intention of the French classification was to keep all electrical measuring instruments for scientific purposes in Group V., a large number of the best have been exhibited by foreign countries—especially by Germany—amongst the philosophical instruments in Class 15, Group III. Amongst these is the Reichsanstalt standard dynamometer.

The instruments manufactured and exhibited by Siemens and Halske, by Jules Carpentier, and for Lord Kelvin by James White, are too well known for their accuracy and perfection of workmanship to require any description on my part. A careful inspection of these exhibits would alone occupy many hours.

After those exhibits to which I have referred as meriting the highest awards, our members cannot do better than make a careful inspection of the exhibits of measuring instruments by Ducretet and by Gaiffe.

The comparison in each branch of this manufacture, which impressed itself on me between 1878, when I was British juror dealing with similar objects and when these manufacturers were in the infant stage of their work, and in 1900, is a startling revelation of the development which one generation has made in these appliances which are now indispensable.

From the point of view of the needs of the electrical engineer, the potentiometers exhibited by Messrs. Crompton, by Messrs. Braun and Hartmann, by J. Carpentier and by ...ux, represent distinct types of form for instruments all ... similar uses. Messrs. Braun and Hartmann's work

is especially noteworthy for its excellence of design and small cost.

But it is in the application of electricity to therapeutics, that this exhibition presents appliances of perhaps greatest ingenuity. Although in the United States invention has perhaps made in the past some original advances in this direction, there is nothing worth mentioning shown at Paris by that country, except, perhaps, an electro-magnet for extracting metallic substances from the human eye, exhibited by E. B. Meyrowitz, of New York.

In the French section of this speciality, however, we find everywhere the triumphant results of the scientific life-work of Dr. d'Arsonval, Director-General in the *Collège de France*, of the senior school and laboratory which teaches biological physics, a school which has been entirely created by that celebrated scientist. The instruments, the origin and design of many of which are due to his researches, are made and exhibited by Louis Bonetti, Bréguet, C. Chardin, Gaiffe, J. Guénet, Radiguet and Massiot, and notably by C. Verdin—all of Paris. Messes. Gaiffe's cage for subjecting the human body to high-frequency radiation, and his self-registering milliampere meter, are worthy of note by the visitor. To these in the same line may be added Hirschmann, of Berlin.

From Germany, in Classes 15, 27, and 30, there are five exhibitors of electro-medical, physiological and biological instruments. Amongst these is the Allgemeine Elektrizitäts-Gesellschaft.

The apparatus for applying currents of high frequency in physiological research, and for radiography, which are exhibited by the firms mentioned above, are the most striking example of the interest taken in the application of electricity to the uses of medical science, as to which there is still so much left to the physiologist to discover.

The absence of Great Britain in this field is so conspicuous, that one wonders if our want of practical enterprise in this direction is due to conditions of humidity of climate, which opposes itself to success without the use of special and expensive precautions.

In Class 27 are included some other applications of electricity to industrial works.

There are exhibits, not however of great note, in con-

nection with time-keeping. 1. Of electric clocks, in which electricity is the actuator of motion in each ; 2. The hourly or daily regulation of any number of clocks connected with a regulating timepiece ; 3. The working of time-keepers or electro-magnetic indicators worked by means of a distributing timepiece. All these are included in the French expression *"l'horlogerie electrique."* A small exhibit of J. J. Stockall & Sons, of Clerkenwell Road, London, which is in Class 96, in the *Esplanade des Invalides*, presents some novel features. Except with what is shown by the Herzog Teleseme Company of New York, the United States, in the forefront of invention, sends nothing. The house of Henry Lepante, 11, Rue Desnouettes, Paris, and the Société Industrielle des Telephones, and d'Arlincourt, show high-class work in these respects. But in front of all, as might be expected, the name of Peyer, Favarger & Co., of Neuchâtel, stands pre-eminent.

The use of electrical appliances in connection with the working of railways and other public works has received great development on the Continent since 1878. Neither Great Britain, or her Colonies, which at that period (when Mr. Spagnoletti was my co-juror) was well in advance in the invention and use of apparatus for railway working, has sent one object in this connection to this (truly) Great International Exhibition at Paris of 1900. The use of solenoids and small motors for working heavy signals and controlling gates at a distance, by energy stored and delivered from accumulators, is illustrated by Jules Guénet and by Postel-Vinay, both of Paris. The latter exhibits in Railways. An examination of what is exhibited by the great French railway companies in this connection causes regret, even for instructional reasons, that England and the United States have abstained.

It is impossible, within the limits of time allowed me, to do more than bring a few of the special applications of the use of electrical energy to notice, and of these there are too few, especially of exhibits of materials used in the electrical industries. Only mica and micanite, one in the French, one in the United States sections, are worth a visit.

Of the materials for primary batteries there is not one important show.

is, however, one interesting departure in electrical

heating and cooking, by Parvillee Bros., of 20, Rue Gauthey, Paris. Where the heat is required, either for warming air in circulation, or in close proximity to the object to be cooked, whether to be roasted, boiled, or braised, resistances made of "metallo-ceramique Parvillee" are fixed, which, by the passage of a current of 30 to 35 volts, are raised at the middle (but not at the ends, where they are held like a safety-fuse) to a condition of dull incandescence. The only description of these that I have seen will be found at page 385 of *L'Electricien* of June 23, 1900. The Jury of Class 27 inspected the use of these at the kitchen of one of the largest restaurants on the Quay d'Orsay, where all the processes of cooking for the *déjeuner* of a large *clientèle* was in progress.

There are several fair examples of lightning-conductor work, and in Class 15 Messrs. Hartmann and Braun, of Frankfort, exhibit apparatus for measuring terrestrial magnetism and its variations, together with a means of testing samples of iron.

There are several ingenious fire and water level indicators; and B. Ougrimoff, of St. Petersburg, exhibits an electric boiler for heating steam to a pressure for house distribution, which might be practical, though costly.

For purposes of Naval equipment Messrs. Siemens and Halske make a very complete show, combining means of controlling artillery and torpedo projectiles, the steaming and the steering from one or several points in a warship, with the use of the pressure (110° volts) employed for lighting the ship as well as by special moisture-proof telephones. Their special arrangements for pneumatically counter-poising heavy lever handles, for acknowledging back all signals and calls, for synchronising and for lighting each handle, dial, and indicator, are well worthy of attention.

NOTICE OF EXHIBITS IN CLASSES 24 AND 25 IN THE PARIS EXHIBITION.

ÉLECTRO-CHEMIE AND ÉCLAIRAGE ÉLECTRIQUE.

By CARL HERING, President of American Institute of Electrical Engineers.

One of the most interesting exhibits is that of the Nernst lamp, which is shown in operation in quite large numbers in the pavilion of the Allgemeine Electricitäts Gesellschaft of Berlin. The filament is made chiefly of magnesia, together with some of the rare earths, like thoria; it is shorter and thicker than in the carbon incandescent lamp. The lamps are now made for 220 volts, continuous or alternating, and for 25 and 50 c.p. and over; they therefore do not compete directly with the low-candle-power, low-voltage, carbon-filament lamp. The efficiency is 1·5 watts per Hefner candle, which is about twice as good as that of the present carbon lamps; besides this high efficiency, it has the advantage of the high voltage. It is made in two forms, in one of which the filament is preheated with a match or alcohol torch, the bulb being open; and in the other it is heated with a neighbouring fine platinum wire, which is automatically cut out of circuit when the filament conducts. The filament has a rapidly falling temperature coefficient, which is a great disadvantage, as it would make the lamp very sensitive to changes in voltage; to overcome this a very fine iron wire is always in series with the filament, and is so proportioned that for the normal current it is heated to a temperature at which it has a very rapidly rising characteristic, so that the lamp as a whole is not very sensitive to changes in voltage; this iron wire absorbs about 10 per cent. of the voltage. The perishable parts are so arranged that they are easily replaceable at a cost of only about 25 per cent. of that of the original lamp.

The Bremer arc lamp is a novel departure of some interest. The carbons contain certain metallic salts like borax, or those of magnesium or calcium, especially calcium fluoride, which, at the temperature of the arc, emit white oxides, that deposit on receptacles near the arc and act partly as a white reflector and partly as a Nernst conductor,

forming a broad illuminated surface, the arc being 4 to 5 c.m. in length. Wedding, who is an authority, found the efficiency to be 0·13 watts per mean hemispherical candle The carbon consumption is about double that of the ordinary lamp.

The Pulsford method of making a vacuum in lamps by means of a chemical which absorbs the residual air, is shown in operation; it is not new. Weismann shows a system of using very low voltage lamps with a small transformer for each group, claiming thereby to increase the light efficiency by about 50 per cent.; it seems very doubtful whether the attending disadvantages are not greater than the alleged advantages. The enclosed arc lamps which are exhibited, are all American or of American origin; four types are exhibited, in one of which a life of 300 hours is claimed: the French claim that the light is too blue and unsteady. A number of arc lamps of interest are exhibited in the French section, and they all seem to burn very steadily. The general type of mechanism is that of the brake escapement, which feeds very gradually; all the lamps are intended to be run on constant potential circuits, generally two in series, but in some cases as many as three, and the lamps are then generally differential, having both a series and a shunt coil; some of them are for so small a current as two or three amperes, and burn very well.

Those interested in search-lights will find some very fine exhibits, chiefly in the French section; also a few in the German department.

The exhibit of chandeliers is almost entirely French, and they are, as a rule, very artistic. They are found chiefly in the Gallery and in the Invalides.

Three underground cable exhibits are shown in operation, subjected to from 25,000 to 30,000 volts, one in the German section, another in the Italian, and a third in the French. In the two former rubber is used, while in the latter the insulation is impregnated paper without any rubber, which adds to the interest of the exhibit.

There are a great many exhibits of meters, but it seems that the well-known Thomson meter is the one chiefly used, the French Company which manufactures it claiming to make about three hundred a day. This Company has made some improvements in it, and also exhibits two-rate meters,

prepayment, and hour meters. It also exhibits the O'Keenan meter, which is of considerable interest, as it is a cheap ampere-hour meter for small consumers; it might be said to be a d'Arsonval galvanometer in which the coil revolves and registers the number of revolutions; it starts with as small a power as one microwatt, and registers as small a current as 5½ per cent. of that of a single lamp; 11,000 are already in use. A modification of the Thomson meter, of some interest, is shown by the Luxsche Works in the German section; the armature is made of three coils like in the old Thomson-Houston arc machine, through which the current flows in series instead of in multiple, as in the Gramme ring form, and therefore the whole armature becomes much lighter, with many attending advantages. The Aron pendulum meter is well exhibited; improved types are shown, including a three-phase meter. The Holden periodically registering meter, which is shown in operation, has several points of interest.

In Class 24 (Electrochemistry) there are a number of fine exhibits, though they are hardly sufficient in number to represent the true state of the art. Among the more important may be mentioned the products of Moissan's classical researches; also his furnace in operation. There are also several carbide furnaces in operation which, together with a very large ozone apparatus for sterilising water, are all shown in the Annexe for Electrochemistry. The Acheson carborundum is shown in the American mining section, and some of the products made from it are exhibited in the electrical section. There are two fine exhibits of the Elmore copper-depositing process, in which the copper is deposited directly as a tube during the refining operation. There are quite a number of exhibits of accumulators, most of which are French, and are all together in one group; there are also several good English exhibits of accumulators. In the French accumulators section there are several which are intended for automobiles, among which may be mentioned the Fulmen, Pulvis, and the Phenix. An electrolytic engraving apparatus for engraving steel dies by means of a plaster of Paris mould saturated with an electrolyte, and lightly pressed against it while the current passes, thus etching away the metal, is shown in operation in the German section. The Goldschmidt process of burning

powdered aluminium with oxides of other metals producing extremely high temperatures and reducing the metal in very pure form, is shown in operation; it is used commercially for reducing such metals as chromium for the iron industry, and for mending broken iron parts of machines, or joining rails for tramways.

The Three Hundred and Fiftieth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 8th, 1900—Professor JOHN PERRY, D.Sc., F.R.S., President, in the Chair.

The minutes of the Annual General Meeting held on May 24th, and of the Extraordinary General Meeting held on August 16th, 1900, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that these names should be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

C. Procter Banham.
F. R. Davenport.
Edward Henry Tyler.

From the class of Associates to that of Associate Members—

Frank Armstrong.
Reginald Henry Tudor Drummond.
Arthur Hattersley.
John Clarence Lyell.
Harold Window Morisset.
Harold Skipwith.
Frederick J. Thompson.

From the class of Students to that of Associates—

William Ernest Few.
Captain Walter John Underwood.
Frank King Westbrook.

From the class of Members of the Northern Society of Electrical Engineers to the class of Members—

A. Lester Taylor.

To the class of Associate Members—

A. Gee.
Walter James Hide.
A. Kelly.

To the class of Associates—

Joseph McDermott.

The PRESIDENT: Before the Secretary reads the list of donations to the Library, Building, and Benevolent Funds, I should like to say that Colonel Crompton, who, as you are aware, has just returned from South Africa, has presented to the Institution an exploder used by the Boers for blowing up bridges.

Colonel R. E. CROMPTON: I have carried this Dynamo Exploder from Nelspruit to this room, which is a journey of nearly 8,000 miles, in order that the corps of Electrical Engineers, who are now on their way home, may present it to the Institution. The remainder of the officers and men will be in London within a month from this date, and then, perhaps, a more formal presentation can be made.

At one time during my journey from Nelspruit to Pretoria the Boers attacked the line just after our train had passed, and then its re-capture by the Boers was very probable.

This piece of apparatus may be interesting to you, as we believe it was the identical apparatus which was used by Théron to blow up and destroy property of immense value in the shape of railway bridges, buildings, waterworks and the like. We thought that such a trophy would be more appropriately presented by members of our profession than the usual fragments of shell, rifles, and the like.

The PRESIDENT: I may say we are to have an even more valuable gift from Colonel Crompton during the session—a paper on his experiences in South Africa.

Donations to the Library, Building Fund, and Benevolent Fund were announced as having been received since the last meeting from—

The Astronomer Royal, Mon. C. Beranger, The Harvey Iron and Steel Company, Mr. J. B. C. Kershaw, The Lords Commissioners of the Admiralty, Messrs. Methuen and Co., Professor Spooner, Drs. Squier and Crehore, Mr. W. F. Stanley; Messrs. E. Danvers, H. F. Parshall, W. P. Maycock, J. W. Meares, T. L. Miller, J. Swinburne, Members; Professor J. Epstein, Mr. M. Otagawa, Foreign Members; and Mr. A. Stewart, Associate. *Building Fund*, from Messrs. E. B. Ward, L. J. B. Wall, Leon Drugman, E. M. A. Malek, W. W. Strode, A. A. Crawford,

J. P. Lawrence, J. G. Wilson, J. H. Garratt, C. M. Renton, A. Wray, C. E. Grove, R. Sykes, J. Grant, John Denham, Sydney Evershed, A. Stroh, A. C. Curtis-Hayward, E. Gimingham, W. O. Rooper, T. H. Harrison, W. E. Langdon, C. Faraday-Proctor, T. W. Tischendörfer, E. Mascart. *Benevolent Fund*, from Messrs. Jonas and Colver, Limited, A. E. Whittaker, F. W. Huband, J. W. Smith, Redfern, Nalder Bros. and Thompson, A. C. Cossor, J. W. Barnard, Thomas Sanders, Alfred Arculus and Co., The British Electric Safety Lamp Works, William Rickard, Webb, Shaw and Co., Taylor and Co., T. W. Lench, Limited, The Campbell Gas Engine Company, The China Furniture and Electric Fittings Manufacturers Association, Davis and Timmins, Limited, Le Carbone, Anderson, Anderson and Anderson, Rainsford and Lynes, F. Krasa and Co., Henderson and Walker, Allen Everett and Co., Hawkins Bros. and Co., J. G. Wilson, The Electric Power Storage Company, "Anonymous,"

to whom the thanks of the meeting were duly accorded.

The PRESIDENT: Dr. Silvanus Thompson will now present the premiums to those who have earned them during the last session, when he was President of the Institution.

The "Institution Premium," value £25, to Mr. C. E. Grove Member.

The "Paris Electrical Exhibition Premium," value £10, to Mr. S. Evershed, Associate Member.

Two Extra Premiums, value £10 each, respectively to Professor George Forbes, F.R.S., Member, and Mr. H. M. Sayers, Associate Member.

A Premium, value £5, to Mr. A. Russell, Member, for an "Original Communication."

A Premium, value £5, to Messrs. C. C. Hawkins, Member, and R. Wightman, Associate, for an "Original Communication."

The First "Students' Premium," value £10, to Mr. R. P. Howgrave-Graham, Student.

The Second "Students' Premium," value £5, to Mr. C. B. Nixon, Student.

The Third "Students' Premium," value £5, to Mr. H. J. Humphreys, Student.

The Salomons Scholarship to Mr. R. P. Howgrave-Graham, a student of the Technical College, Finsbury.

Colonel R. E. CROMPTON: The first duty that has been entrusted to me on returning to this country is the congenial one of proposing "That the best thanks of the members of he Institution of Electrical Engineers be given to Professor vanus Thompson for the very able manner in which he filled the office of President during the twelve months,

1899–1900, and for the indefatigable personal attention which he has given to the duties of the office."

It is unnecessary for me to remind you of the valuable services which Dr. Thompson has rendered to our profession by his writings, teaching, and inventions; I think you must take this for granted.

It seems only yesterday that I was in Switzerland with that extremely pleasant party over which Dr. Thompson presided with such courtesy and success, and I must remind you that this first and highly successful experiment of an extended Continental tour owed its success very greatly to our President; it was very fortunate for us that he was a *persona grata* with every one we met in our tour through the towns and works of Switzerland. He was specially gifted by his linguistic attainments and by his power of expressing himself in German so as to be able to speak for our Institution in a graceful and dignified style, which all who heard him felt proud of.

Mr. W. M. Mordey: I have very great pleasure in seconding Colonel Crompton's resolution. On account of Colonel Crompton's absence from England he has had perhaps fewer opportunities than those who have stayed at home of seeing the work Professor Thompson has done for this Institution. All here know with what urbanity, care, and skill he has conducted the general meetings. But the President of this Institution has much important work to do besides that at the general meetings. His part in our first visit abroad you all know. I need hardly tell you that in committee-room work, and the work which has to be done in organising the arrangements, and dealing with the many matters that come before the Institution, Professor Thompson has also rendered the highest service. I have missed few meetings of this Institution for about nineteen years, and so I have had some experience of our Presidents. Comparisons are to be deprecated, but I think I may say we have had no more successful President than Professor Thompson. I am sure every future President will feel that Professor Thompson has made it a difficult task to adequately occupy the Presidential chair.

If we had more time to-night, I am sure, as your applause shows, you would like to spend some of it in expressing more fully our appreciation of all the services which Dr.

Thompson has rendered this Institution. They have culminated, but they have not ceased, I hope, in his occupancy of its chair.

The resolution was then put by the President and carried by acclamation.

Dr. SILVANUS P. THOMPSON : I thank you very heartily for this vote of thanks, particularly for the kind, the too kind words, in which it has been moved and seconded. Every President in his turn has to do his duty, and I have tried to do mine. The statistics of the Institution show that we are not yet at our highest point of attainment, for the curve of increase has still an increasing upward slope. It is quite clear that we are making additional progress every year. The fact that in my year of office the increase has been higher than in any previous twelve months, is of course due to the good work that has been done for years past by those who have preceded me in the chair of the Institution, and to the work of the officers of the Institution, the co-operation of the members of the Council, and of the membership generally, working together for the general good. Evidence of this can be seen not only in the statistics of membership but in the facts as to our resources. For example, it is a most gratifying thing to me to have been President during the year when we have added no less than £2,140 to our Building Fund. Of that sum, about £470 was actually subscribed last autumn in consequence of an appeal we sent out ; about £181 is brought in by interest in the stock already invested for the Building Fund, and £1,400 was transferred from the surplus of our general fund about a year ago. The Benevolent Fund in like manner has grown by about £470, of which £30 only is dividend on stocks held for the Benevolent Fund. Further, during the year that has gone by we have had that splendid gift from Dr. Henry Wilde, our Honorary Member, of £1,500 to form a special Wilde Benevolent Fund. We have also been the recipients of a most handsome legacy of £2,000 from our beloved late past-President, Professor Hughes, to found the David Hughes scholarship. All this is evidence that our Institution is making good and substantial progress. Its activities are greater and greater every year ; and knowing the character of the work that is undertaken by the Council and the spirit in which that work is carried out, I, in retiring

from the office of President, am confident that I may leave the chair with the satisfaction, not merely of looking back upon the prosperous year during which I had the honour of occupying it, but of looking forward to future years when this Institution will enjoy even greater success and attain even greater influence. Particularly confident may I feel that the chair is now occupied by one so well capable of maintaining its dignity and its credit, and of wielding righteously, and for the good of the electrical fraternity, its ever-increasing influence.

I have had the pleasure and satisfaction of knowing Professor Perry personally for twenty years, and for twelve years of that time I was intimately associated with him as a colleague in daily intercourse. And I will say this, that no man on earth could wish for a truer friend or a more completely perfect colleague to work with than John Perry, your present President. Therefore I now perform this last duty, on retiring into the position of ex-President, of congratulating you on your choice of my successor.

INAUGURAL ADDRESS.

By PROFESSOR JOHN PERRY, M.E., D.Sc., F.R.S., President.

I do not intend to make this in any sense a report of the progress of our Institution during the last or any number of years. I shall not, therefore, give any account of the exceedingly good work done by Colonel Crompton and the active service corps of our Electrical Engineer Volunteers in South Africa. I shall not describe how we *fêted* our American cousins in England and France, nor how they *fêted* us; nor what a wonderful success accompanied all that was attempted by us or by them or by M. Mascart and our French colleagues, although I cannot refrain from bearing my testimony to the great kindness of the Prince of Wales and the British Commission in so generously lending us the British Pavilion for our great reception and giving us the use of one of its rooms for our office all the time of our visit to Paris.

My brother has tried to get me to introduce to your notice

some novel ideas which have come to us during the last ten years in our business of lighting the City of Galway from a fairly constant water-power using accumulators with a Dowson gas plant stand-by. It has almost come to be a practical idea to produce Carbide of Calcium in wet seasons and utilise it through the gas engine in dry seasons. I was also tempted to discuss the use of large gas engine plant at central stations; and another of several subjects in which I have been recently engaged has been the magnetic effect produced by systems of electric traction. But I have resisted temptation and have chosen a subject which seems to me much more important.

Your president's address is followed by no discussion. He is, therefore, privileged, but his very privileges cause him to address you with a greater sense of responsibility; he may say what he pleases, but he must be very sure that he has the best interests of the Institution at heart; the interests of the Institution as a whole, not the interests merely of a few members, and least of all ought he to think of his own interests. Nevertheless, your president speaks not as an omniscient judge, but rather as a very fallible, very prejudiced one-sided man who, because he has devoted himself to one part of the work of this Institution, is certain to be unfair in his comments upon other parts of the work.

Your past presidents represent in this way all classes of members of this Institution. You have had scientific men, given, some of them to calculation and some to experiment and some to both; men who have advanced the study of pure science. You have had practical telegraph men, civil and military, men cunning in land and deep-sea telegraphy and telephony; men cunning in railway signalling. You have had electrical chemists. You have had manufacturers and users of all kinds of electrical appliances. You have had men who devote themselves to the teaching of electrical engineers, and who fully appreciate the fact that no good teacher ought to be out of practical touch with the profession. And nearly all your past presidents have invented things which are now in practical use.

As each of these men has given you at least one address written from his own peculiar point of view, his prejudices are not likely to have done any harm to members who read

the other addresses. I know, therefore, that you are good-naturedly prepared to give me plenty of rope. I can predict the twinkle of amusement in the faces of some of my friends when they learn that I am about to take up a subject on which we have had many debates. Your attention has just been attracted to the curves showing the numerical growth of our Institution in membership, and this has been marvellous, but I would speak of something more than mere numbers, namely, the *quality* of our membership. In fact, I mean to put before you this simple question: Is electrical engineering to remain a profession or is it to become a trade? Is this Institution to continue to be a society for the advancement of knowledge in the applications of scientific principles to electrical industries, or is it to become a mere trades union?

Of course, at the present time the outside public are willing to regard membership of this Institution as a Symbol of something more than the membership of a mere trades union. During the early growth of any trade, even such a trade as that of the plumber, it was really a profession. And a common trade may suddenly become a profession, if it suddenly begins to develop, as, for example, stone-masonry of a hundred years ago suddenly developed into civil engineering. Electrical engineering has been developed rapidly, so that in the past it has certainly been a profession and not a trade.

Again, we are an institution of engineers, and the general public are willing to class us with other engineering institutions—for example, the Institution of Civil Engineers Now the title M.Inst.C.E. is a professional distinction which represents in civil engineering what F.R.C.S. does in surgery, or M.R.C.P. in medicine. We owe a great deal to our association with, and recognition by, the Institution of Civil Engineers; our meetings are held in its rooms; many of our members are also its members; our proceedings are modelled on its proceedings.

Now this older Institution, governed by the best thoughts of the best British engineers, has laid it down that its associate members, that important class from which the higher class is mainly fed, shall have passed certain specified examinations in pure and applied science.

I am not now suggesting that we ought to adopt this

science examination method of admitting any kind of members to our Institution. I do not believe in the wholesale adoption of methods of working from another society. I am asking you early in my address to remember that this greatest of all professional engineering institutions, governed by practical men full of common sense, knowing the wants of their profession well, insists upon a knowledge of science in its new members. If this recognition of science did not exist anywhere else in the whole world, I say that its recognition by such a thoroughly good professional society as that of the Civil Engineers ought to recommend it to all professional societies.

In Germany an enormous stride has recently been made in the raising of Engineering degrees to rank with the highest University honours. There is hardly one engineer of eminence in Switzerland, France, or Germany who has not passed with honour through the classes of one of their great science Universities.[1] In Great Britain within the last fifteen years not only have great engineering schools been established in all the manufacturing towns, but even in Cambridge University there is one of the best schools of civil, mechanical, and electrical engineering of which I know anything.

Before we think of imitating the Institution of Civil Engineers, we ought to reflect on certain fundamental distinctions between that Institution and our own which at first sight seem to make us less professional.

There is a well-known unwritten rule of the Civil Engineers to which there are only a few exceptions, that no contracting railway or harbour engineer can acquire the title of M.Inst.C.E. I think myself that it is a pity to draw a hard and fast line between consulting engineers and contractors. No doubt it simplifies the labour of the Council in its selection of candidates, but it gives rise to anomalies.

A man who was once a civil engineer because he served a

[1] I understand also that the great unions of manufacturers in *Germany* are about to make facilities for giving a year of real factory work to the Polytechnic students, thus perfecting the German system. In Japan we found great success in requiring students to spend their summer in real shops, their winters at college. In England it may be that we shall prefer to let all apprentices have shorter factory hours than workmen, articled pupils much shorter hours, their masters being responsible for instruction being given in theory.

pupilage under his clever father, and who now is nominally at the head of his father's large practice, the real engineering work being done by many clever employees, this man may be a member. A contracting engineer who shows marvellous ability not only in rectifying the mistakes of the designer of a large bridge or tunnel or reservoir embankment, but shows the power of Lord Kitchener in directing the work of thousands of men so that no man need be idle, and the whole contract goes on like clockwork, and is finished well in the minimum of time, this man is ineligible. Now in our Institution it has been recognised from the very first that manufacturers and contractors and their employees may belong to the very highest ranks of their profession. Of course, I do not mean men who simply receive the profits of businesses, or even men who merely work to obtain orders for themselves. I mean men who are not merely formally but in reality manufacturing or contracting engineers. I mean men who, in dealing with standardised things, design new methods for quick, good, cheap production of such things. I mean men who improve old forms of things, possibly through their paid subordinates. I mean by a manufacturer fit to be a M.I.E.E a man who might act as his own manager, and who, perhaps, has a wider outlook than on mere managerial duties. So long as a contractor or manufacturer is really an engineer, we know that we add to our strength with the addition of every such member.

But consider a contractor who only uses ordinary types of machines or electrical plant in well-known ways, surely he can hardly be said to be in the profession at all. Surely the one thing that differentiates us from mere tradesmen is that we do not follow mere rule of thumb methods; we think for ourselves, we weigh advantages and disadvantages. If every new installation required the same treatment as existing ones, the engineer would degenerate into a tradesman, and it seems to me that the electrical engineer ought to have a special fear of such degeneration.

In railway and harbour and river and sanitary engineering, in every new job there are new difficulties to be dealt with. An engineer who designs many undertakings and sees them carried out must be a thoughtful man; he cannot

help keeping himself acquainted with engineering principles, and so he is a professional man. So an architect finds that each new job requires all his experience. Every case that comes before a real physician or surgeon requires a somewhat different treatment from any old case. Every case brought before a barrister requires the exercise of all his past experience. In every case a *profession* implies the necessity for the exercise of all the outcome of one's past experience; because the work one has to do is never the same as any work one has ever done before. And when I say the outcome of past experience, I really mean certain general principles which one has always in one's mind, principles derived from all that one has done or seen or read about.

Electrical engineering is in a curious position. It owes its being altogether to scientific men, to the laboratory and desk-work of a long line of experimenters and philosophers. Even now the work going on in a laboratory to-day becomes the much larger work of the engineer to-morrow. When at length the laboratory experiment is utilised in engineering, we see that there is no other kind of engineering which so lends itself to mathematical treatment and exact measurement. Most of the phenomena dealt with by the electrical engineer lend themselves to exact mathematical calculation, and after calculations are made exact measurements may be made to test the accuracy of our theory. For a completed machine or any of its parts can be submitted to the most searching electrical and magnetic tests, since these tests, unlike those applied by the mechanical engineer, do not destroy the body tested.

Contrast this with the calculations it is possible to make in other kinds of engineering. The pressure of earth against a revetement wall is possibly 200 or 300 per cent. greater, or 50 to 70 per cent. less than what we imagine it to be in what some limited men call theory. We use factors of safety 5 or 10 or more on all kinds of iron structure calculations, because we are aware of our ignorance of a correct method of dealing with the problems. The civil engineer never has exactly the same problem as has already been solved. In tunnelling, earthwork, building, &c., in making railways and canals, he is supremely dependent on the natural conditions pro-

vided for him: the configuration of the surface of the ground, the geological formation, the structural materials available in the neighbourhood. The story of how the engineer has to study the endlessly different ways of interaction of water and sand and gravel is told by the troublesome bars at the mouths of rivers all over the world, by the difficulties of coast and river-bank protection, by the failure of sea walls and piers. But why should I make a catalogue of the different kinds of work done by civil engineers? Every one of them needs the exercise of general scientific principles due to much experience.

Now of all such natural difficulties the consulting or contracting electrical engineer is greatly independent. Give him a source of power and tell him what is to be done; whether he is to light a town or a building, whether with arc or incandescent lights; whether he drives a stamp mill near a mine or a pump, or a machine tool, or a spinning frame, the electrical part of the work is carried out in much the same way. Natural conditions affect him mainly in the cost of transport of his materials and the cost of labour. He can make in an easy way the most careful calculations as to the best arrangement of his conductors and machines to give maximum economy, and except for this easy calculation his work is that of a mere tradesman. He is practically independent even of the weather. There are, indeed, some of us who grumble that this easy calculation is not made easier still, who prefer to make arithmetical guesses rather than exact calculation, because perhaps we like to see a little uncertainty introduced into the problem to make it more like a problem in civil engineering. I want members to see clearly that as time goes on, as our electrical engineering work gets more and more cut and dried, the man who loses the power to calculate, who loses his grip of the simple theory underlying our work, must sink more and more into the position of a mere tradesman who has no longer the right to call himself an engineer.

An electrical engineer must have such a good mental grasp of the general scientific principles underlying his work that he is able to improve existing things and ways of using these things. It has become the custom to call this *theory*, and I suppose I must follow the custom. I should

prefer to call it *Science*[1] or *knowledge*. Do you remember Huxley's definition of Science? "Science," he said, "is organised common sense"; and this is really what I mean. Well, calling it *theory*, the man who is permeated by theory, whose theory is so much a part of his mental machinery that it is always ready for practical application to any problem, he is the real engineer. But you must not mistake me in this matter. Eighty per cent. of the men who pass examinations in mathematics, mechanics, and electricity have very little of this theory. Fifty per cent. of the writers of letters in the engineering journals in which mathematical expressions occur have almost nothing of this theory in their possession. It is unknown to foolish men. Books alone, lectures alone, experiments alone, workshop experience alone cannot teach this theory. The acumen of a Q.C. may actually prevent a man from acquiring it. A man may have much of this theory, although he may never have listened to lectures, although he may dislike the sight of a mathematical expression. I have known men who might be called illiterate to possess much theory. I have known many men who might be called good *electricians* who are almost wanting in the theory necessary for the electrical engineer.

I am speaking only of theory. Of the other qualifications for an engineer I need not here speak; they are present to the minds of all of us. A man may have any amount of knowledge; he may know how to apply his knowledge, and yet he may not be able to apply the knowledge from a want of engineering *character*.

The engineer must be a real man; he must possess individuality, the power to think for himself. He must not be like a sheep, knowing only enough to follow the bellwether. Over and over again in the last thirty years have some of us given our students much the same sort of advice

[1] What Doll Tearsheet said of the word "occupy" we have to say of the word "Science." It is used by many people out of its proper meaning and then condemned, so that one is getting afraid to use it. In Prof. Fitzgerald's splendid inaugural address to the Dublin Section of this Institution he says: "As has recently been pointed out to me by Dr. Trouton, it would be impossible to say the same contemptuous things of *knowledge* as are said of *Science*. In Germany the word used, 'Wissenschaft,' is the one corresponding to our word 'knowledge,' and there nobody of any sense could say that 'knowledge is all humbug,' as is here often said, and still oftener thought, of 'Science.'"

that Baden-Powell gives to scouts in that excellent little book of his. If any of you have not read that book you ought to buy it at once, and you will there find that if a man is to think for himself he must possess all kinds of knowledge, he must be constantly picking up new kinds of knowledge.

Nobody can limit the value of any kind of knowledge, but still one may say that certain things are probably more important than others. To gain what we call "theory" a good general education is most helpful — mathematical knowledge is very helpful; laboratory and workshop experience are extremely helpful. There is one qualification which the electrical engineer must have and without which all other qualifications are useless, and if a man has it no other qualification is supremely important, and this absolutely indispensable qualification is that a man shall love to think about and work with electrical things. He must like these not because of the money he can make through electrical contrivances, nor even, I think, because of the name he may make before the world—this would be mere liking or cupboard love which has no lasting quality. So long as we have men in this country who have the true love for scientific work of which I speak, so long shall we have a real profession of electrical engineering, for such men are always scheming new contrivances and improving old ones and utilising the services of all helpful people, and especially of capitalists. When we have reached a state in which nobody schemes new things because the existing things are perfect there will no longer be a profession of electrical engineering. Of all ideas surely that of having reached *perfection* is most hateful: the idea of exact knowledge, that nothing is unknown, that there is no need for thought and therefore that to think for oneself is a sin.

And so, although we are all agreed that much standardisation in our contrivances and methods is absolutely necessary for our competition with other nations, we must follow the Americans in this matter and take care that it does not destroy invention. Of course when things are really standardised, when we have our perfect Mauser rifle or dynamo or locomotive or traction engine or electrically driven stamp mill, a Boer can buy or even manufacture them if he has money, and he can use them as well as, or possibly better than, we can. But he is not an

engineer. He uses things after the engineer has done his work upon them. A stoker, a common engine-driver, the guard of a train, these are not engineers. You must have noticed that the American engineers, who surely deserve the character of being practical idealists above all other engineers, are the men who are most imbued with notions of standardisation which lead to cheapness of manufacture, and they are also the men most alive to the necessity for occasional extensive scrapping of types of machinery when they become even a little antiquated.

Our chiefs, the men who run us all, our real men at this Institution, may be called Practical Idealists. They have imagination and judgment and individuality. They have the imagination and enthusiasm of inventors, and yet they are more than inventors, for they can estimate the worth of their own inventions and control their imaginations. They are ready to receive all new ideas, they welcome all new things, and yet they are not carried away. They are radicals and yet they are conservatives. They have what Mrs. Beecher Stowe called *Faculty*.

A strong imagination well under control, surely it is the greatest of mental gifts. I look round me and wonder how many of us really have it; and how many of us are only dull men, who scorn novels and poetry, who live utilitarian, material lives, whose aim is merely to make money through electricity, who love it not for its own self, who cherish their "tuppeny-ha'penny-worth" of theory because it is sufficient for their immediate wants. Why, even the writers of leading articles in the daily papers can talk of the wonders of electricity and what may yet come to pass; and yet we who make machines and use them and switch the marvellous thing on and off and take all sorts of liberties with it—we are like Calibans oblivious of the wonders of the fairy isle—like soulless priests making a living in the temple of Isis—like Aladdins who rub our lamp only to get the necessaries of life.

Twenty years ago some of us were laughed at for our optimism, and yet everything that we declared then to be doable has now actually been done by engineers, except the thing which was then and is now declared to be the supremely important thing, namely, the electric consumption of coal. We say now, as we said then, "The applied

science of the future lies invisible and small in the operations of the men who work at pure chemistry and physics." And think of the wonderfully rapid rate at which laboratory discoveries have been made in the last eleven years, and how as the years go on they become more and more numerous; and yet many of us plod along with our work seeing no farther than our noses. A year is now more pregnant with discovery than a hundred years used to be, and yet the protective stolidity of our ancestors is upon us and we think of the latest discovery as if it were really the very last that can be made. A thousand men are measuring and trying new things in laboratories all over the world. Some of them plodding and soulless; others of them with imagination and clearness of vision. Do you think that nothing is to come from all that work?

And is it not one of the most important functions of the engineer to do as Mr. Marconi has done, to convince capitalists ignorant of science that if the successful laboratory experiment is tried on the large scale it must also be successful? And are we going to leave all this pioneering work, with all its possibilities of great gain, albeit with possible loss, to foreign engineers, when in most cases the scientific discovery has been made in England? Are we so lacking in the hope and faith which are born of imagination and science? And must we in the future as in the past have to rely upon the influx of the clever foreigner like Sir William Siemens? Must we, Boer like, always depend upon our Uitlander population, Fleming and German, Hollander, Huguenot and Hebrew, for the development of our natural resources?

Some of the best engineers I know are so exceptional that one must class them with geniuses; they have faculty and character, and so they have become engineers even under the most unfavourable circumstances. They have passed through ordinary schools and yet developed common sense. They were pitchforked into practical work, and their liking for the work as well as some curious kind of instinct led them to pick up all sorts of knowledge which have become part of their mental machinery. They continue to pick up new kinds of knowledge when these become necessary for their professional work. Unfortunately these men do not realise

how exceptional they are, and they advise boys to go direct from school into works. They forget that the other 99 per cent. of men treated in the same way as themselves can only become the hewers of wood and drawers of water to real engineers. Treated in this way average boys are just like so many sheep: they learn just what seems absolutely necessary and no more; their acquaintance with the scientific principles underlying their trade is a hand-to-mouth knowledge which becomes useless when their trade undergoes development.

In 1867 I was an apprentice, and when in the drawing office and pattern shop I remember well how I was chaffed for studying such a non-paying, non-practical subject as electricity. When I published my first electrical paper in 1874 before the Royal Society, and even for some years afterwards, the real students of electricity in England could be counted on the fingers of one's hands. Many of us remember the first Gramme magneto machine that came to this country, a scientific toy, in 1874. How many engineers dreamt that a great new branch of engineering had been started? Even in 1878 engineers were as a rule quite ignorant of electricity, and since then every year, although newspaper writers have talked largely of the age of electricity, the men actually engaged in electrical industries have acted as if the greatest of changes were not perpetually going on in it. To be left behind, or to become camp followers, children of Gibeon, this is the usual fate of the men who scorn theory. In 1882–4 we used to have to pay men £200 and £300 a year because they had a slight knowledge of electrical matters. In 1884–6 these very men were not worth twenty shillings a week, they were weeded out of the profession and their places were taken by men of better knowledge. Two or three years after, these better men were again found to have been weeded out, because men of still better knowledge were available. And so it has gone on ever since. Men learn just enough to get posts; they settle down in these posts and scorn theory. They actually forget what little theory they once did possess. They know a great deal about existing machines, but presently they discover that improvements have been going on, and that they no longer have a right to say that they belong to the engineering profession. In every year one has told

men, "You will be left behind. See A and B and C. I told them three years ago, when their names were in everybody's mouths, that they would be left behind like their predecessors, and they laughed. Now I tell you and you laugh, and you also will be left behind. Yes, I know that you get a good salary or large fees, and your head touches the sky. Nevertheless, because you neglect theory and the simple mathematics by means of which theory is made available in practical problems, you will have to take a back seat presently, for our profession is in its early youth and is growing rapidly."

Remember that I do not now refer to the few exceptional heaven-born engineers who, in spite of bad training, do manage somehow to pick up the necessary knowledge. I speak of the average men, many of whom are now living in the same old fool's paradise. They know enough for present needs; they scorn the simple principles which underlie all our work; they scorn the easy mathematics by which these principles are most readily employed in practical problems; they will have their reward.

Just think of what is occurring at the present time. In England we have cheap coal, and it can be carried easily. In Switzerland and other countries where there is no cheap coal the water-power had to be utilised and power had to be transmitted great distances electrically. This needed high voltage, and as it is difficult to get high voltage with direct current machines, alternating currents were used, and on account of motor troubles multiphase working has been introduced. What a revelation it was to almost all of us, that visit of a year ago to Switzerland! We saw enormous schemes of lighting and traction and power. We saw electric trains driven by distant waterfalls sandwiched in among ordinary trains keeping proper time on working railways. We had known that there were great schemes carried out in Germany and America and other countries, and yet all the machines were quite unfamiliar to us. We were very much like what engineers of 1870 would have been if suddenly brought into a generating station. Is it not a fact that some of us, said to be eminent and thought to be practical, asked questions and made remarks which showed that we did not know the most elementary principles of three-phase working. Is it

then any wonder that the traction schemes now being developed in England, on lines that are certainly not the best for this country of their adoption, are altogether dependent on the use of foreign electrical machinery and employ foreign electrical engineers? I am not putting this altogether fairly, for municipal procrastination has prevented our development, and yet I am not putting it altogether unfairly. We know too little theory.

I am afraid that just now we are in a rather tight place. I would give something to know how we in this room are going to get acquainted with what some people rightly or wrongly consider the most important kind of modern electrical engineering. Our usual way of learning is by actual handling of things. But if the millions of pounds' worth of machinery coming to England every year is all foreign and is used mainly under foreign superintendence, our usual method of study is made very difficult. True there are American and German, and indeed a very good English publication which would give a knowledge of the theory, but not, I think, to the average English electrical engineer. I know of many men 25 to 40 years of age who seldom come to our meetings, and who say they are silent in discussions because they cannot be understood; perhaps these men will find a way to save us all from being left behind. There is much more that I might say in this connection. An individual Englishman may be left behind other Englishmen, and all English electrical engineers may be left behind the rest of the world, but all electrical engineers of the world may even be left behind other appliers of science. It is not merely that the incandescent mantle of the gas engineer is improving and necessitates improvements in our filaments, but in spite of the flourishing conditions of our factories just now, I could give many other illustrations of how we shall all suffer if we do not keep adding to our knowledge. Twenty years ago, when giving some lectures in Clerkenwell to workers in the then flourishing watch trade, I ventured to prophesy the decay of that trade. But I am afraid that the case of Jonah and Nineveh is the only one in which prediction of disaster led to reform. I venture on no prophecy therefore, because it might harden your hearts.

Much of the evil we suffer from is due to our average

young men being pitchforked into works where they get no instruction, as soon as they leave school. If ordinary school education were worth the name, and if schoolmasters could be brought to see that we do not live in the fifteenth century, if boys were really taught to think for themselves through common sense training in natural science, things would not be so bad. But the average boy leaves an English school with no power to think for himself, and with less than no knowledge of natural science, and he learns what is called mathematics in such a fashion that he hates the sight of a mathematical expression all his life after.

And what is the result? English engineers do make a wonderfully intimate acquaintance with the machines and tools that they work with, but when it comes to the manufacture of new things they do it by fitting and trying, by quite unnecessary expenditure of money through trial and error. A machine is made and tried and then another better one, until a good result is arrived at. And this method did well enough in the past and would do well enough in the future if only we had not to compete with foreigners who can really calculate. It is not all smoke; there is a real danger in this foreign competition unless we mend our ways. There is an absolute necessity for great change in English ways; but there are so many people interested in the maintenance of old methods of working; so many people who think they will lose their bread and butter if a change takes place; so much capital, scholastic and other, invested in our old machinery; that it takes a catastrophe to produce changes. Much of the strength and weakness of England has always lain in her conservatism. We have been talking of *standardisation* of machinery lately, so I may say that things have been standardised in England for a long time. Now to get all the good effect of standardisation it is occasionally necessary to go in for wholesale *scrapping*, and it is this scrapping part of the business that we dislike in England. We here all know that the District and Metropolitan railways might have been worked electrically years ago just as easily as they will be when we are allowed to begin upon them, but of course the scrapping of a lot of steam locomotives was a serious thing. The loss of experience to English

electrical engineers, because of this hatred of scrapping, is leading to other incalculable losses. I understand that the whole generating and line plant—the whole machinery of the Boston tramways—has been scrapped several times since they first were driven electrically. Japan has scrapped all her old civilisation just as France did. During the century now dying Germany has made the most sweeping changes in her land and school legislation, and indeed in everything. England and Spain and China, how they differ in this respect even from England's own colonies.

Of course it may be said that English customs have grown during centuries; they are well tried and there is no pressing need for sudden alteration. I quite agree, but unfortunately this very perfection and fitness of our customs have bred in us a want of flexibility, so that in cases where a sudden change is really necessary, we are disinclined to make the change merely because it is a change and for no other reason.

No one has ever heard me speak of the decadence of England. When the greatness and the wealth, the manliness and the strength, the healthiness and good life of England are shown forth to the as yet ignorant world in all their magnitude there will be some astonishment. But it is our duty to keep up our high standards. We must change what is bad when we know it to be bad, and not let bad things[1] continue to exist, parasitic growths, maintained because on the whole we are strong and healthy. You will perhaps think that this is a very serious exordium when I tell you that I have introduced it all on account of the state of mathematics in our profession. I feel a sort of degradation every time that I hear a successful, clever old member of this Institution sneering at mathematics. There is a plausibility about his statements; he himself has been very successful in life without much help from mathematics; but indeed his sneer is doing a great deal of harm to the younger members who admire his success, who forget that he has

[1] Such as our wretched system of weights and measures. Oh young America and Australia, is it wise to waste a year of every child's life, and years of the life of every business man, merely because we do it in England? ... get many of your pedagogues from us, and of course they say that with- ...ts., qrs., and lbs., and Latin declensions and Euclid, the mind cannot be ... Do you believe them, or are you with open eyes making a great ...ntal sacrifice?

succeeded in spite of, and not because of, his neglect of mathematics.

Our knowledge of electrical phenomena must be quantitative to be of practical use; we must be able to calculate. Mathematics is the science of calculation, and we must therefore be able to employ, and we all do necessarily employ, less or more mathematics every hour of our professional lives. The draper and the grocer and the housekeeper merely need arithmetic. Everybody now knows some arithmetic. Everybody can add and subtract and multiply and divide, and keep accounts in some simple sort of way. This is due to the fact that arithmetic is no longer taught in the old Greek method with its twenty-seven independent characters (for our ten figures), the study of which required a lifetime, so that only old men could do multiplication, and they not only needed many hours to do one easy bit of multiplication, but declared that if the art were not practised every day it could not be remembered. Reading and writing and ciphering are now taught to everybody. It used to be that only learned men and philosophers could read, write, and compute. You will remember the charge that was brought against one of Shakespeare's characters, who was said to possess mere bookish theory without practical knowledge. "And what was he?" "Forsooth a great arithmetician." Nowadays, when everybody can compute, we should say of the possessor of mere bookish theory, "Forsooth he knows the calculus."

For in mediæval times things were taught in such a way that only a few men had a chance of knowing how to read, write, and cipher. We have been compelled to change all that, the pedagogue has by compulsion given up his mediæval methods of teaching in these things, although in all other matters he retains them. But a time has come when we see that ciphering is not enough mathematics for us to be familiar with, we need a little algebra, we need co-ordinate geometry, we need the differential and integral calculus. The pedagogue tells us that we must follow the orthodox course of study, which takes many years; and some of us, many of us, who have followed the orthodox method find that we have spent so much time and mental power upon it and its thousands of unnecessary tricks and contrivances and philosophy, that we can take in no more

ideas. We cannot utilise our mathematics on engineering problems because we are too old and tired and *blasé* to comprehend these problems. Nevertheless we are the only people who know mathematics, and so we publish volumes of unmeaning and useless disquisitions on problems that we do not understand. Or we know just enough mathematics to be able to show our ignorance to experts, but quite enough to impress engineers with our knowledge; and we know just enough about engineering problems to show our ignorance to engineers, but quite enough to impress mathematicians, and what we publish is merely as the crackling of thorns under a pot.

As for the man who does understand electrical problems, he remembers that there was a something called a study of mathematics at his school, that he did pass certain examinations with much difficulty and tribulation, that the subject had no real meaning to him even when he was supposed to know it, and he now hates the sight of anything that looks like mathematics.

I tell you, gentlemen, that there is only one remedy for this sort of thing. Just as the antiquated method of studying arithmetic has been given up, so the antiquated method of studying other parts of mathematics must be given up. The practical engineer needs to use squared paper. What is the use of telling him that he has taken an unauthorised way to the study of co-ordinate geometry, that he cannot approach it except through Euclid and modern geometry and geometrical conics and algebra and trigonometry. He says the youngest child can be made to understand diagrams on squared paper.

So again the idea underlying the calculus is one that every child, every boy, every man possesses and uses every day of his life, and there are useful methods of the calculus that might be taught quite quickly to boys, and which it would be a pleasure to boys and men to use continually in all sorts of practical problems, but of course the subject of the differential and integral calculus is one that must come at the end of a long course of what is to the average boy utterly uninteresting and unmeaning mathematics. Indeed, the average boy never reaches the subject, whose very names, differential and integral calculus, are enough to drive him frantic.

Yes, the schoolmasters say that we must follow the mediæval rules of the game, and all sorts of fine things are said about them, but as a matter of fact we only need to bring a little common sense to bear upon schoolmasters. At present most of us stick to our arithmetic as a safe and well-tried friend. We compute after the manner of the draper and grocer and housekeeper. In finding out what is the best size of conductor, or armature winding or core, or iron and winding of a field magnet, we calculate by mere arithmetic for one size and then for another; perhaps we have weeks of arithmetical computation before we find the right size of thing to use, and we cannot frame general rules. And some foolish person who knows a little mathematics, works at the problem (as we ought to be able to do but are not) and he frames a general rule and we laugh at it, and sneer at mathematics because he has probably left out of account the most important consideration. We know that the result is wrong but we cannot say why it is wrong.

Then there are some far-reaching, labour-saving ideas that we simply cannot get into our heads at all, we cannot comprehend them. Am I sinning against the rule as to good comradeship which exists here if I say that some of us are ignorant of the most fundamental fact regulating economy in arranging sizes of conductors? Suppose we find the total cost of installing a conductor of a certain length, using one square inch section of copper. We do the same thing for other sizes, and we plot total cost and weight of mere copper on squared paper. I do not care what system we adopt if it is the same system for all sizes, and if we buy our materials from the same manufacturers and use the same kind of labour, our points will lie very nearly in a straight line on the squared paper. Hence increased cost will be proportioned to increased weight of copper, and, indeed, increased total cost will be like the mere increase in the cost of copper, taking a slightly higher price of copper per ton. Some of us, ignorant of the elementary mathematics involved in the problem, think that the mistake has been made of assuming that the cost of an installed conductor is merely the cost of the copper in it, and of course we must feel that it is too absurd a mistake not to be laughed over. With an elementary knowledge of mathe-

matics our mistake would be impossible, and without such a knowledge the clever electrical engineer is constantly discovering mare's nests in the investigations which he criticises.

I know of long misleading accounts of the results of good experimental observations which might have been described in a few clear words by the aid of elementary mathematics. I know men who spend on a particular problem ten times the amount of worrying thought that would enable them to master the easy mathematics that includes all such problems. Quite recently one of our most eminent members declared to me that he had not really grasped the reason for small economy at a power station when there is a small load factor until he studied the common sense mathematical form which has been given in a recent publication. And yet he is a man who has heard much, and read much, and talked much on this subject.

Every electrical engineer has a correct idea of how a transformer acts, or how the E.M.F. in one of the coils of an armature of a direct current or other generator, or, let us say, a rotary transformer, changes during a revolution, and how the E.M.F.s of all the coils are combined to produce currents in the external circuits. But through how much mathematical tribulation must most of us have passed from our state of ignorance to our present state of knowledge! It is no wonder that we are disinclined to the study of a new phenomenon which seems as if it might lead us through the like tribulation. The tribulation is least because it is suffered only once if we first learn the Calculus method which underlies all our work; it is greatest if we get it up in a completely new-looking form in every new problem. I speak now of what is most difficult in our study, for there is thought required in applying the Calculus method. Thus, for example, in multiphase work at the present time the best mathematicians wonder how it is possible for easy calculation to be made in such a subject. What we want just now is that an electrical engineer
a th three-phase current phenomena should
naster of ordinary easy mathematics that he
discovering a very simple way of putting the
. At present calculation is easy but tedious,
pellent; but I am perfectly certain that a

competent man might quickly invent methods of calculation which are not only easy, but short and thinkable. Mathematicians with the requisite electrical knowledge, again, may be lacking in sympathy and humour. I know a book of more than three hundred large pages on ordinary alternating currents, and all the information in it is given far more simply in two pages of another book with which some of you are acquainted. Possibly, just now, mathematicians who are electrical and who have common sense have too much other work to do, and we must wait their leisure.

The fact is, mathematics ought to be the natural language of the electrical engineer, and at present it is a foreign language; we cannot read or write or think in it. We are at the beginning of our development, like monkeys whose necessities have increased faster than their powers of speech.

Some of you are aware that a new method of teaching mathematics has recently been introduced by the ever-to-be-praised Science and Art Department in nearly all evening classes in science schools throughout the country.[1] I wish I could say that there was a prospect of its being introduced in all schools, for it seems to me that this would lead to the result that all young men entering works would be masters of that kind of calculation which is most important in electrical engineering; not merely a few men having this power, but the average men, just as average men can read and write.

I am addressing engineers, men who utilise the results arrived at by scientific workers, men whose profession is applied science. But surely if we are to apply the results arrived at by scientific men, if the laboratory experiment of to-day is to be our engineering achievement of to-morrow, we ought to be very much alive to all that is going on in the scientific world.

All men ought to be far more alive to the importance of scientific work. On the psychological side, it is perfectly exasperating to me to see how few are the men who know that Darwin has given a key to almost all the great philo-

[1] See summary of Lectures on Practical Mathematics; also the Science and Art Directory, and the Reports of Examiners on the Science Examinations of 1899 and 1900, all published by the Education Department, South Kensington, S.W. The reforms now advocated in mathematical and science teaching are all clearly described in a paper read before the Society of Arts in January, 1880.

sophical problems of antiquity, and that there is a great mental development accompanying the more evident engineering development now going on in the world. Again it is the fault of our methods of education that all our great men, our most important, most brilliant, best educated men, our poets and novelists, our legislators and lawyers, our soldiers and sailors, our great manufacturers and merchants, our clergymen and schoolmasters, should remain so ignorant of physical science, the application of which by a few men not ignorant is transforming all the conditions of civilisation.[1] But of all men just think what it means for engineers to be ignorant of science, or neglectful of its new developments, and of all engineers think what it would mean if electrical engineers sinned in this way.

Except ours, all other branches of industry have taken thousands of years to grow. There were bridge and hydraulic and sanitary and harbour and river engineers in ancient Rome, and such engineers existed thousands of years before the first papyrus was written in Egypt. But no Assyrian tile or Egyptian hieroglyphic or relic from a tomb indicates that telephones or electric motors or electric lights existed before our time. No gradual improvement in our methods of conquering nature led up from small beginnings in our electrical engineering. Our profession has not grown during thousands of years of time like other professions. It has sprung suddenly, full grown, from the new spirit which is going to rule the souls and bodies of men, the spirit of research in pure science. The new spirit puts knowledge, mere knowledge of nature, as its highest aim. The scientific student knows that all sorts of good must come to mankind from his studies; all sorts of scientific knowledge are sure to be utilised by engineers, but in the pursuit of science the usefulness and utility of the result are of no importance. And are we—we who have received the first-fruits of the labours of scientific men, we the first-born spoilt children of the great parent of all that is to come, we who form the foremost files of the present time—are we going to turn upon our beautiful young mother and say she is useless and ugly, and she hinders our money-making, and that we are willing to kill her for the sake of the burial

[1] See articles in *Nature* of July 5th and August 2nd.

fee? Thank God that is the spirit of only a few of us. Have we not as an Institution gone to great expense in the publication of *Science Abstracts* in partnership with the Physical Society? That publication has been and continues to be of the very greatest value to all students of pure and applied science who read our language, for it tells them the results of all the scientific work now being done in all parts of the world. And even if some of us do not read that useful publication, do we not know that it is there to read if we like? Do we not know that it is a symbol of our redemption from the yoke of the Philistine? It is one of many signs that in answer to the question which I have asked in this address, we can truthfully say that we are professional men, that our profession has promise of enormous expansion and improvement, and that we are not likely to become mere tradesmen.

I am afraid that you will think that I have a personal interest in putting before you the claims for consideration of the pursuit of pure science, because you know that I am trying to defend Kew Observatory from imminent danger. In truth I have no interest in this matter unbecoming a president of this Institution. For two years I have been trying to reason with traction engineers. Like many other electrical engineers these gentlemen desire to use uninsulated return conductors. If they do so near a magnetic observatory certain records of terrestrial magnetic disturbances are quite spoilt. At Potsdam this sacrilege has been forbidden. At Washington, Toronto, Capetown, and most other important places the magnetic records have already been rendered useless. Professor Rücker and I were asked by the other members of the Committee of the Royal Society which was in charge of the Kew Observatory to defend Kew, and with the help of Her Majesty's Treasury we thought we were able to insist upon the use of insulated returns in all undertakings authorised by Parliament where harm was likely to be inflicted on Government observatories. I may say that the scheme designed by Mr. Clifton Robinson for using an insulated return conductor in the working of the tramways of the London United Tramways Company, in consequence of our action, was a thoroughly good scheme which it gave one satisfaction to look at, not ugly and not expensive. It

seemed to me a fit scheme for any tramway system, however complex, in which overhead conductors are used. You are aware that for an electric railway or for a tramway where an underground conduit is employed, it is in every way better, and it is in a large scheme actually cheaper to use an insulated return. We felt therefore very happy, for magnetic observatories seemed quite safe from interference. We were, however, mistaken, for the only clause which we have been able to get inserted in all Parliamentary authorisations of undertakings, leaves it to the Board of Trade to substitute other methods of protection than the insulation of the return conductors in cases where these other methods seem to be sufficiently good for the protection of laboratories and observatories, and this is why the Board of Trade appointed the Committee which met on the 31st of October probably for the last time.

Professor Rücker, Professor Ayrton, and I have made many tests on the magnetic disturbances produced by tramways and railways, particularly by the Stockton tramways and by the Waterloo and City Railway, and we have had many meetings with the traction engineers, but nothing has yet been decided.

I mention this matter, which has given great anxiety to scientific men, because I am afraid that some of you may think when you hear of it that I have been acting against the interests of the electrical industry. I beg to assure you that I have been acting in your best interests. As an electrical engineer I ought surely to regret the use of uninsulated returns even if we leave Kew Observatory out of account. Suppose we do not now insulate our returns. Electricity will certainly return by gas and water pipes, and the amount of harm done to those pipes is merely a question of time. Because of the ignorance of legislators and gas and water companies, nothing is said just now, but will nothing be said at the end of ten or twenty years when pipes are found to be eaten away everywhere? And if by a slight increase of expense, or rather, as I think, actually no increase of expense, but merely a little increase in inventiveness and common sense on the part of electrical engineers, this evil may be entirely prevented, surely it is in the interests of all of us that insulated returns should be insisted upon. But even if we do not insist on insulating

the returns in all systems, surely something may be said for the giving of this protection on lines near such a magnetic observatory as Kew. Even the magnetograph records now being made have been continuous for forty-five years, and if Kew is interfered with no sum of money can compensate for the interference; for if the Observatory were removed the future observations would have no link with the past.

An engineer in this room declared that it seemed to him an injustice to hamper the progress of electric tramways "for the sake of making observations that never have given, and never may give, to the world any important results." Now, it is not so much on account of Kew that I object to this sort of observation, as for its general spirit of antagonism to scientific research.

There is no doubt that the answer to the old question which Gilbert might have asked three hundred years ago, "What is the cause of terrestrial magnetism?" is very jealously hidden from us by Nature. The earth probably contains much iron, but its great internal heat seems to forbid our imagining the iron to be magnetic. The assumption that a negative electric charge on the rotating earth will explain things, requires such an enormous charge that this assumption has been discarded. There are annual and diurnal variations of a fairly regular kind; there are storms which have some relation to the Aurora Borealis, to sun-spots and to earth currents. There are small sudden changes which seem to occur almost instantaneously all over the earth. Observations of these things may be useless from some points of view, but scientific men have been and continue to be willing to give up time and much money for this object. Utilitarians had to be cajoled through superstition to allow observations of the stars to be carried on in ancient times, and we have no such cajolery to offer. We simply say that it has been through this sort of useless-looking method of working that all our progress in science has come.

Engineers descended from men who sneered at Cavendish and Franklin and Volta and Oersted and Ohm and Faraday, are you who utilise the results of the work so sneered at and pile up fortunes in consequence of it, are you the men to sneer at and ridicule the scientific work of the present day because it seems to you useless?

Tell us a better method of observation; give us better suggestions as to what these magnetic phenomena may mean; but the past record of scientific observation enables us to laugh at you when you say that magnetic observations may never give the world any important results. Was Nature ever so open and yet so closed about a secret as she is about this one of terrestrial magnetism? Was there ever one whose revelation promised so much? How very little we know of electricity and magnetism! Does the mere motion of the earth, taking no account of electric charges at all, cause it to be magnetic? Almost anything is on the cards. Surely I need not appeal to your cupidity, but it is quite possible that our knowledge of this secret may enable us to tap a tremendous store of Nature's energy.

Gentlemen, this is not a trades union, and it is not a society for the furtherance of pure scientific research, but it is a society of professional men who recognise the past services of scientific observers with gratitude and respect, and hope for greater ones in the future. And shall it be said of us that our gratitude is not greater than that of Judas, to whom indeed thirty pieces of silver was doubtless a large sum; that "we have given our hearts away a sordid boon;" and that as to our future hopes we are willing to sell our birthright for a mess of pottage?

Professor GEORGE FORBES: I am much gratified in being permitted to propose "That a hearty vote of thanks be offered to Professor Perry for his most interesting address, and that with his permission the address be printed in the Journal of the Proceedings of the Institution." I think you will all agree with me that you have a President who has shown by the splendid address which we have just heard that he is a man who can form his views in an independent spirit, that he will bring them forward at the right time in an effective manner, and has no fear of their being pleasing or displeasing to anybody in the world as long as he feels he ought to put them forward. As long as we have such men among our body he need have no fear that our engineering will fall behindhand. We shall, so long, have men who will have the boldness, the imagination, and the spirit to carry out new engineering works in the face of the world. I have seldom listened to a more interesting

address than the one that is concluded, and that has been only too short. There is a great deal that Professor Perry has kept back that he can still produce on the same strain, I am perfectly sure. At the same time I am sure that each one of us here has found much in the address which he would like to argue out with Professor Perry quietly over a table. This is not a fitting occasion, however, to argue the various points, but there is no doubt that every single point which has been raised by Professor Perry is one worthy of discussion, and one that each of us would benefit by thinking out thoroughly for himself, and seeing it in the new light in which it has been put before us by Professor Perry. He has told us that the three classes who are necessary to the furthering of our profession are in danger of falling behind—the consulting engineers, the manufacturers, and the capitalists who own the plant. I think we must all feel that there is this danger, and that if we do not bestir ourselves we shall be running a serious risk of being left behind. I could say a great deal of the conclusions one has arrived at on these points, but this would not be the time to do so. I certainly agree with a great deal of what Professor Perry has said on these subjects, and on certain others I hold different views which I should be very glad to talk over with him some other time.

I think it augurs well for the state of the profession in this country that the address which we have heard has been listened to, not only with such rapt attention, but with such manifest approbation by so large an audience of engineers here. In the earlier days of electrical engineering I do not think that Professor Perry would have been applauded so much when he spoke about the benefits of theory. The period, however, is past now when engineers despised knowledge, when it was the commonest thing in the world for an ignorant man to brag about it and say, "Oh, I am not a theorist; I do not know anything about theory. I am a practical man." What an assumption for any man to claim for himself such a splendid title! The practical man is the man who knows everything, and who has imagination, and resource, and the readiness to apply it when it is required. It was the most egregrious act of assumption to have claimed such a title, and there are few engineers who, in the fullest sense, deserve that splendid title of being practical men.

Professor Perry has made that as clear as possible to-day, and the advance in thought that has come of late years has shown that these views are appreciated, and that knowledge, rightly applied, is at the basis of success in engineering.

So also we must appreciate the value he has attached to pure physical science, to laboratory research, and still more to mathematics, which is a tool that enables us to put our knowledge to practical use. I think it was Sir William Siemens who told us that he considered mathematics were a very good servant but a very bad master. Mathematics is really a process of logic, and when your premises are given, your mathematics ought to give the correct deduction and the right conclusion. The difficulty is in getting your premises. Lord Westbury once said, with regard to logical argument, to a rising barrister, "Never be in fault in deriving wrong conclusions from facts : the facts are at your own disposal."

I think we must all have felt, in listening to this address, that there was something that inspired us and filled us with admiration, besides the mere words which were uttered, and that is the obvious thought that had been required, the long period of thought from which this address resulted. We felt that it was evidently the result of years of thinking, and the obvious sincerity and earnestness of the speaker convinced those who might not have held the same views before they came into the room. Again, the charm of language and the telling way in which the facts have been put before us have raised this address very far above the average. In fact, it has confirmed the opinion which, I suppose, is pretty universal among literary men, viz., that the finest, most enthralling, most imaginative and poetic work that has been published in the lifetime of any of us, after the "Prisoner of Zenda," is Professor Perry's book upon spinning-tops.

Dr. SILVANUS P. THOMPSON : This notice will be seconded by our oldest past-President present, who happens also to be the oldest living telegraph engineer, Mr. Spagnoletti.

Mr. C. E. SPAGNOLETTI : It gives me great gratification and pleasure to have the opportunity of seconding the vote of thanks proposed by Professor Forbes. We are all very much indebted for the lesson we have received to-night. I hope all our young members will lay it to their hearts, and

profit by what they have heard, so that they may get their reward. Professor Perry is a man of great energy and power. I believe it is only yesterday he was down at Newcastle, throwing his mantle over the northern branch of our Institution, and assisting in the business there which proved such a very great success. The labours of that day were heavy upon him. He had to be up again early to-day, and after his journey he is here to-night to give us the benefit of his advice. I have great pleasure in seconding the vote of thanks.

The motion was carried by acclamation.

The PRESIDENT : I can only say that it has been exceedingly good of you to listen to me with so much attention. I knew that you would, whatever I might say and whether you approved or disapproved, but I did not expect so much enthusiasm. It was especially good in Professor Forbes to speak as he has done, and it has been very pleasant to listen to Mr. Spagnoletti. Of course I know perfectly well that when you have time to reflect you will like to argue out this whole business, for there is plenty to be said on the other side of each of the questions I have raised. But then we are always saying those other things, whereas to-night I wanted to say *these* things.

When Colonel Crompton and I bicycle together in the morning before breakfast, we take opposite sides and sometimes we change sides in discussing these questions, and I have discussed them with many others of our prominent members, and all through this address I have let you see how well I know that there is another side.

Mr. Spagnoletti has referred to the Newcastle local section. Gentlemen, I have great pleasure in going to see these colonies of ours, and the Newcastle one is as interesting as any. We sat down to dinner last night, about sixty of us; Mr. Heaviside, who has come all the way to hear this address to-night, was in the chair; and a better looking set of heads, engineering heads, thoughtful heads, it would be very difficult to find anywhere. I congratulate you on your colony at Newcastle, and I congratulate myself on not being quite too tired with my two days' journey to read my address to you. I thank you again for your attention and applause.

The Three Hundred and Fifty-first Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, November 22nd, 1900 —Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 8th, 1900, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that they should be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Francis Gibson Baily.

From the class of Associates to that of Associate Members—

Ernest Denn Long.

Messrs. R. Tervet and E. Brothers were appointed scrutineers of the ballot for the election of new members.

Donations to the Library and the Building Fund were announced as having been received since the last meeting, to the Library from Mr. E. Garcke and to the Building Fund from Professor Epstein, Mr. T. Cushing, Mr. W. von Siemens, Captain E. W. Creak, Mr. R. von Boschan, Mr. R. Portelli, Mr. W. M. Mordey, Sir Henry Mance, and Colonel R. E. Crompton, to whom the thanks of the meeting were duly accorded.

The following paper was then read :—

TELEGRAPHS AND TELEPHONES AT THE PARIS EXHIBITION.

By J. GAVEY, M.Inst.C.E., M.I.E.E.

The successive Universal Exhibitions that have been held in Paris since the year 1878 may not inappropriately be regarded as milestones along the road of progress in Telegraphic and Telephonic industries, for each in its time has not only afforded unrivalled opportunities for the study of new work, but collectively they offer conclusive evidence of the value of the various discoveries or fresh departures that have been made from time to time in various fields of research, and the degree of success that has been achieved by past inventors and workers may usefully be laid to heart by those engaged in similar efforts at the present moment. Thus, for example, glancing back at the reports of the Paris Exhibition of 1878, there will be found in the list of telegraphic exhibits the Hughes type-printing, the Baudot Multiple, the Duplex, the Quadruplex, the Wheatstone, and other systems which have achieved permanent success, as is forcibly illustrated by the State Telegraphic Exhibits of working apparatus in the 1900 Exhibition, in which they all appear as instruments in daily use. On the other hand, many apparently promising inventions which were prominent in the 1878 and in successive Exhibitions have either dropped out of sight altogether or are still in the course of development—so far, at all events, as actual practical use is concerned.

Class 26 in the catalogue includes Telegraphs, Telephones, and Phonographs, and I propose to deal with the sub-divisions in this order. Further, I may add that in the brief review that I have undertaken of the exhibits in this class I propose to dwell only on those which show considerable novelty, or which illustrate the progress made during the last decade, rather than to attempt an exhaustive description of the class as a whole.

Under the head of Telegraphs there are three distinct systems of Telegraphy which deserve special mention, each exhibiting considerable merit from the scientific point of view, although possibly they may be unequal from the

practical standpoint. They are designed either with a view of increasing the amount of traffic which can be carried by a telegraph circuit, or of abolishing the transcription at the receiving instruments necessary with Morse methods; but they are based on very different principles.

Rowland's Multiplex.—The Rowland Telegraphic Company of Baltimore, Maryland, exhibit an apparatus of the multiplex type of working which will admit of eight messages being sent simultaneously, four in each direction. The apparatus was at work in the Exhibition, and we were told that satisfactory results had been obtained over a distance of six hundred miles of actual line in America. This is so far in excess of the mileage over which we have been able to work in this country with Delany's Multiplex, that a brief explanation of the principle of multiplex working generally may fitly precede a description of the new invention.

Multiplex telegraphy is based on the fact that the carrying capacity of an overhead telegraph circuit is far in excess of the speed at which a telegraphist can manipulate his key. For example, a wire which, worked by automatic Wheatstone, could dispose of three hundred words per minute, if worked by hand can only carry on an average thirty words per minute. The inventors of the Delany system of Morse multiplex have therefore designed a method of increasing the carrying capacity of a wire, while retaining the ordinary method of key manipulation, which is based on the following general principles: Two commutators, each divided into a considerable number of separate segments, are fitted at opposite ends of a circuit. Groups of these segments are connected in proper sequence to the number of instruments to be worked simultaneously, while between each group certain segments are reserved for the maintenance of synchronism. The brushes to which the line wire is connected are caused to revolve synchronously by suitable means, and as they sweep over the segments they complete the telegraphic circuit for a very brief interval through three, four, or six corresponding pairs of instruments manned by separate operators—first, No. 1 instrument of one terminal station being connected to No. 1 instrument of the other; xt, No. 2 to No. 2, and so on. These successive connec- follow one another so rapidly that, although the initial

current necessary to signal a dot or a dash in the Morse system from No. 1 instrument at one end to No. 1 instrument at the other may be interrupted four or five or six times, these interruptions are of such brief duration that they do not affect the continuity of the signal received, any tendency in that direction being counterbalanced by suitable methods. In this manner, with the Post Office Delany system, six messages may be simultaneously in course of transmission over the same wire without the signals interfering with each other, so long as the synchronism of the revolving brushes is maintained. This being premised, if the electric current traversed the line from end to end without retardation, there would be no limit to the distance over which this method of signalling would be available. Unfortunately, however, the current is retarded between the forwarding and receiving stations, this retardation being largely due to the electrostatic capacity of the line, in combination with its resistance, and although in practice suitable arrangements provide for a certain amount of retardation of the line current, still a point is soon reached beyond which satisfactory working of the existing Multiplex system becomes practically impossible, the current say from No. 1 instrument at one end arriving at the other end after the line has left No. 1 instrument and has been connected to No. 2.

A modification of the above system, designed by Mr. Pollock, of the General Post Office, is intended to obviate to some extent this difficulty. By an improvement in the method of synchronising which abolishes hunting, and by dividing the commutators into two concentric series, one for transmitting and the other for receiving, and mounting them so that one series may be revolved relatively to the other by a variable adjustment, it is possible to arrange that the receiving contacts shall lag behind the transmitting ones for exactly the period taken by the current in traversing the line. This method is still under experimental trial, but it is at present only adapted for Morse signalling.

The Rowland apparatus has the following promising characteristics :—

Messages are transmitted from several sets of keyboards of the typewriting character, and any typewriting clerks should, with a little practice, be able to manipulate them.

The messages are received on instruments which print them in ordinary type on a long roll of paper, which is perforated at convenient intervals to facilitate division when the received messages are printed.

The electrical apparatus consists of an alternate current dynamo as the source of power, from which a continuous series of electrical undulations traverse the circuit when no printing signals are being sent.

The synchronism between the receiving apparatus and the alternating currents, on their arrival from the distant office, is provided for by means of a small continuous current motor, to which is rigidly geared a little alternator, both fixed on a shaft in the same axial line as the main driving shaft of the receiver. A circuit in which are two condensers, which are alternately charged and discharged from a battery by the to-and-fro movement of the main relay tongue actuated by the received alternations, is so connected up with the alternating motor on the receiver, that when the received undulations from the line and those due to the local alternator on the receiver are in unison, only a continuous uniform beat is heard in a telephone used as a synchronising detector. The speed of the receiving alternator is varied, by the insertion of resistance in the motor driving it, until this result is arrived at. Once attained, synchronism is maintained automatically by the local alternator acting either as a dynamo or motor, according as the speed of the shaft tends to advance or recede. The main driving shaft on which the printing mechanism is mounted is revolved by a second independent motor, and by suitable adjustments of the resistance in this motor circuit the correct speed is arrived at. This done, then so long as the two shafts run with absolute uniformity a little jockey wheel connected to the first rides on a small insulated point in a disc attached to the second. If the main driving shaft lags, the jockey wheel makes a brief contact which reduces the resistance in the circuit of the driving motor, thereby increasing its speed, whereas if it advances a second contact is made which energises an electro-magnet and establishes a magnetic break by the generation of eddy currents in a copper disc revolving between its poles. The synchronising of the two sets of apparatus, therefore, is a relatively simple matter; and as the receiving apparatus is synchronised

so as to run in unison, not with the distributing discs at the forwarding end, but with the retarded currents which arrive at the receiving end, there is no reason to doubt the possibility of working for much longer distances than is possible with the original Delany system.

Geared on to the shaft of the sending alternator is a distributing commutator with fifty-two segments, and four groups of eleven consecutive segments are connected each to a set of eleven levers actuated by the transmitting keys. The odd segments serve other purposes. A second series of four equal segments are connected each to one of the four sets of keys, and these acting on a special electro-magnet on the keyboards admit of any depressed type key actuating the corresponding levers at the right periods only.

When no type keys are depressed, the alternator sends a continuous series of undulations to line, whilst the depression of a key by actuating two out of the eleven levers on the keyboard causes two half waves, always with a complete undulation between them, to be cut off, and this suppression of current actuates the receiving apparatus.

The receiving relay has two tongues: one, already referred to above, serving to establish and maintain synchronism; the other actuating either the printing relays or those for shifting the paper. Each of the four type receivers has eleven electro-magnets in the local circuit of the second tongue of the line relay, each electro-magnet being connected to a segment of the receiving commutator which corresponds with, and revolves in synchronism with, the main commutator on the transmitting dynamo. When no signals are being sent, the currents from the line relay pass through the local relays in such a direction as to keep the tongues against the spacing side, but the omission of two half-waves causes the tongues of the two corresponding relays to drop over to the marking side, and this acts on the electro-magnets which print the corresponding letters, or which shift the paper, as the case may be.

A revolving cylinder, with a transverse mark on the transmitting keyboards, keeps the typewriter at the far end informed of the position of the receiving roll, and advises her when to advance it vertically and shift it horizontally so as to commence a fresh line.

It is stated that each operator can send at the rate of

thirty words a minute, so that with duplex arrangements a speed of 240 words should be obtained, with the advantage that the messages are detached from the receiving instruments in a condition to be sent out for delivery, without the necessity for transcription that the existing method of multiple Morse working of course involves.

Mercadier's.—Monsieur Ernest Mercadier, of Paris, exhibits his multiple telegraph system, in which it is said that twenty-four messages can be simultaneously transmitted over one circuit, twelve in each direction. It is based on the harmonic system of telegraphy, one form of which was devised by Elisha Gray many years ago. Mercadier's method, however, is not a copy of Gray's, as by the use of telephone receivers and transmitters combined in various ways he has designed an absolutely independent method. Harmonic telegraphy depends on the fact that if a number of vibrating reeds, each differing by a certain defined period, say of a musical note, be so connected that each in the course of its vibrations causes a series of currents to be sent into a line wire, the resulting current so formed consists of a series of irregular but well-defined curves, which are due to the combination of the whole series of vibrations emitted by the different reeds, just as in a musical note the sound curve is not a simple one, but is that due to the fundamental note on which are superimposed the overtones. At first sight it would appear as though it would be impossible to dissect the combined current curves, due to the superimposed currents, into their initial undulations. In practice, however, if each of the receiving reeds or telephones, joined up at the receiving end, be tuned to exactly the same pitch as its corresponding transmitting reed at the far end, the receiving reed will respond to the current of the corresponding transmitting reed and to no other, and even though the whole of the transmitting reeds are worked simultaneously, each being used for sending separate Morse characters, the respective receiving reeds select the Morse characters of the right note and disregard all others.

As transmitters, Mercadier uses electrical vibrating reeds of a well-known character, and as receivers he uses a combination of a telephone and a microphone, the latter ›ut a powerful series of undulations into the local

circuit, these undulations being selected by suitably designed telephones, which only respond to the vibrations to which they are tuned.

The method, therefore, is a Morse method pure and simple, in which, so far as the operating is concerned, the telegraphist uses a Morse key for signalling, and the receiving telegraphist uses telephones as sounders. I learnt, whilst in Paris, that the apparatus had, in the course of certain trials, worked satisfactorily between Paris and Bordeaux, but it requires a metallic loop for thoroughly satisfactory working.

Virág and Pollak.—Messrs. Virág and Pollak exhibit their photo-autographic telegraph apparatus, in reference to which so much has appeared in the public press.

The original system, as exhibited in the Hungarian section of the Exhibition, consists of a telephonic receiver, to the centre of the diaphragm of which is attached an arm which conveys the vibrations of the diaphragm to a small mirror. A spot of light falling on the mirror is reflected on to a photographic band, and when the instrument is started any movement of the mirror is photographed on the band. Morse signals are transmitted by an automatic method, and the record is impressed on the photographic band in a series of curves representing the Morse alphabet. It is claimed that an abnormally high speed can be attained, but the system had this disadvantage, in common with ordinary Morse methods, that the whole of the transmitted matter has to be deciphered and written out by the receiving telegraphists.

During the meeting of the Electrical Congress held in the month of August, however, a paper was read describing a most ingenious modification of this system, by means of which the telegram is received not merely in arbitrary Morse signals, but in ordinary written characters.

In the modified system a metallic loop is employed, but it is joined up so as to form two circuits, Fig. 5, one being the ordinary metallic circuit, and the second a bridged and superimposed earth circuit of the type well known in telephony. To each of these separate electrical circuits is connected a telephone with a mounted mirror, which is vibrated by the movement of the diaphragm. These mirrors are so placed in relation to one another that a

spot of light is reflected first on one, then on the other, and lastly on the photographic record. The function of one of the mirrors is to receive impulses representing the vertical component of ordinary written Latin characters, whilst the second mirror receives the horizontal components. The transmitting is arranged by means of a punched slip with

PERFORATIONS.

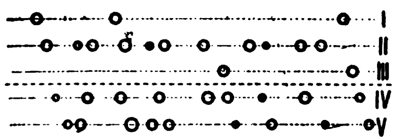

FIG. 1.

VERTICAL.

FIG. 2.

HORIZONTAL.

FIG. .

RESULTANT

FIG. 4.

five series of perforations; the first three transmitting currents to the vertical component mirror, and the second two to the horizontal one. The line of perforations marked 1, Fig. 1, transmits a negative current of a defined voltage; row 2, a positive current of the same voltage; and row 3, a positive current of double the voltage. Rows 4 and 5 transmit through the horizontal component telephone '- reversals.

Two sizes of holes may be perforated along either of the rows, thus admitting of long or short contacts, and by means of a specially designed combination of perforations the vertical and horizontal components are so co-ordinated as to admit of the reproduction of written characters at the receiving end. In the diagram, Fig. 1 illustrates the perforations necessary for the transmission of the word "Telegraph"; Fig. 2 shows the vertical components; Fig. 3 the horizontal; and Fig. 4 the resultant.

TELEPHONES.

The advance in telephonic practice has, on the whole, been greater than in the branch of telegraphs devoted to the transmission of public messages. The invention and perfection of the manufacture of dry-core paper insulated cables has resulted in the more important of the telephonic administrations, both in the Old and New Worlds, undertaking the substitution of metallic in the place of earth circuits for telephonic intercommunication. With the means at their command at an earlier period telephone engineers were practically restricted to overhead work at their large exchanges, and although they realised the disadvantage of earth circuits, few of them could face with equanimity the doubling of the huge number of overhead wires necessary for metallic loops. Now a dry-core cable which will serve two hundred subscribers may be drawn into a 3-in. pipe, and in the course of a few short years an earth circuit telephone will be a thing of the past. Not only has this resulted in a silent and undisturbed subscriber's circuit, but it has admitted of the introduction of improved telephones and improved switching arrangements. With an earth circuit a low-power microphone had necessarily to be used in order to diminish the disturbance on neighbouring wires, for obviously the greater the current emitted, the greater the mutual induction; but with metallic loops improved telephones of the granular type have been generally introduced. The placing of the wires underground has likewise admitted of increasing the number of subscribers connected to each central switch, and this has led to improved methods of automatic signalling.

There is, perhaps, one point in modern telephone work which may here be referred to with advantage, and that is

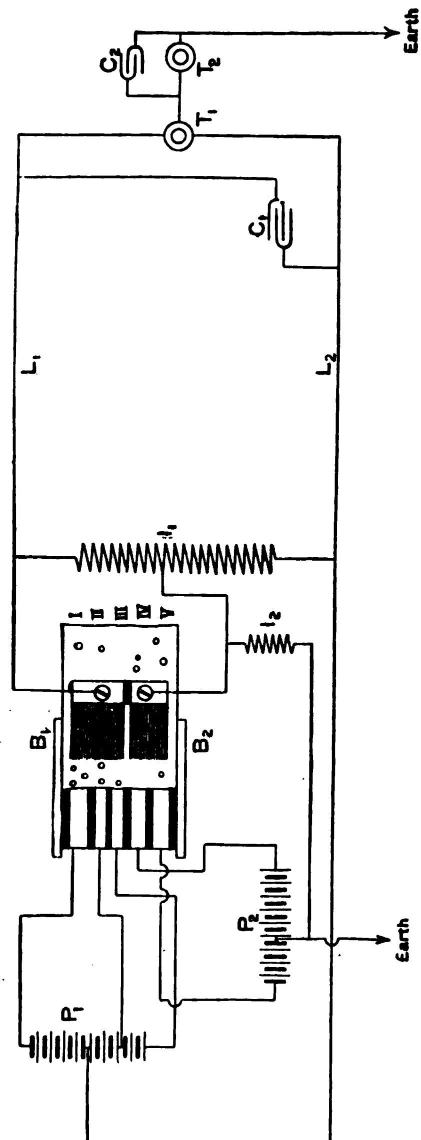

FIG. 5.

the general tendency in the direction of the introduction of matic signalling in modern exchanges. When first the

Post Office opened its local exchanges in the provinces Sir William Preece insisted on the need for this method in order not only to simplify the work of the subscriber, but to ensure the sending of a ring-off signal to indicate the close of a conversation.

At that date automatic signalling could only be effected by the use of primary batteries at the subscriber's office, and objection was taken to the method on account of the alleged expense of these batteries. Given, however, the necessity for maintaining a couple of speaking cells at each subscriber's instrument, the extra cost involved by the addition of two or three more cells for signalling purposes was not so great as was imagined, for a considerable portion of the total maintenance cost arose from the loss of the men's time in locomotion. The principle has, however, spread, and most modern installations at present comprise some method of automatic calling and clearing signals. The diminution in the work thrown on the operator by the use of a well-designed method of signalling, which reduces the speaking to the mere words, "Number, please," "through," or "busy," is highly advantageous in respect both of economy of working and in facility of manipulation.

Central Battery Telephone Switches.—A central battery automatic signalling exchange switch, exhibited by the Western Electric Company, illustrates the system that is being rapidly extended throughout the whole of the United States, and which has been adopted by the British Post Office in London, by the National Telephone Company in some of its centres, and by the Belgian Administration in Brussels. The system has been described recently by myself and others, and space forbids a complete technical description which would demand a separate paper for due consideration. For the purpose of discussion, however, it may be stated generally that the advantages claimed in favour of the system are broadly of the following character :—

(1) The generation of current in an economical manner at the Central Exchange for speaking and signalling purposes.

(2) Uniformity in the character and volume of articulate speech owing to the speaking current being always necessarily maintained at the point of highest efficiency.

(3) Economy of maintenance owing to the absence of

primary batteries, and the reduction of the parts liable to get out of order at the subscriber's office to the minimum.

(4) A complete system of automatic signalling which reduces the need for the operator's intervention to the lowest degree.

(5) The use of small glow lamp signalling indicators which admits of each indicator being placed in immediate proximity to the jacks or cords to which the indications refer.

(6) The possibility of using coloured lights to indicate special rights, such as toll rates, flat rates, deposit accounts for trunk conversations, &c.

(7) The use of a telephone meter for registering the number of conversations initiated by toll-subscribers. This method is not necessarily confined to central energy switches.

Postel-Vinay Switches.—The French firm of Postel-Vinay exhibited a system recently introduced at the newest of the Paris exchanges near the Avenue Breteuil, the most striking feature of which is a remarkably small and well-designed automatic indicator which can be placed immediately over the jack for working the junction circuits. A set of these is shown on the table, but I think that signalling lamps are on the whole preferable to electro-magnetic indicators.

Siemens and Halske.—The firm of Siemens and Halske likewise exhibit a flat board of considerable capacity with certain details of signalling which are of interest. Time does not admit of a general description, but there is one point of novelty to which reference should be made—that is a combined local subscriber's jack and indicator. The call is indicated by the appearance of a small disc at the mouth of the jack. The disc is attached to the extremity of the armature of a Hughes electro-magnet. It is held in position by the magnetism of the core, is released by the reversing action of the current indicating the call, and is restored to its normal position by the mechanical insertion of the answering peg. It is, of course, again held in position by the permanent magnetism of the retaining electro-magnet.

Again, an ingenious method of automatic ring-off is provided by means of a local battery joined through the ...r at the exchange, the effect of which is neutralised

by an opposing polarisation cell at the subscriber's office, which is in circuit when the speaker's telephone is off the hook. At the close of the conversation, however, and on the restoration of the telephone, this second cell is out of circuit, the local battery in the exchange actuates the indicator, and the connection is severed by the operator.

It may be observed that Messrs. Siemens & Halske in their descriptive pamphlet specify the following conditions for satisfactory exchange working :—

1. The Exchange should possess the greatest possible facilities for accommodating new subscribers, hence :

(*a*) The jacks must be as small as possible, yet not so small as to impair their insulation and durability.

(*b*) All other parts must also be very small and compactly built (the indicators are placed out of reach and are self-restoring).

2. To economise expenditure for working, fitting, and space, the manipulation required must be reduced to a minimum, so that one operator may attend to as many subscribers as possible. To achieve this,

(*a*) The indicators must be self-restoring, and the operator's speaking apparatus must be placed in circuit automatically.

(*b*) The clearing signal must be perfectly certain in its action, and must not depend on the subscriber.

Party-line Telephones.—Several forms of party-line telephone circuits have been designed and exhibited, *i.e.*, arrangements designed to serve several offices on one circuit. These are used largely on light railways to provide communication between the headquarters and the various stations on a circuit, each being called by a step-by-step arrangement at will, without disturbing the others. The step-by-step switches which are placed at each office are actuated by battery currents of one polarity, and when these switches are in the position indicating the number of the office required, a reverse current is used for ringing up the "Wanted" subscriber. A needle on each of the dials indicates the number of the office that has been called, and by so doing it further announces the fact that the line is engaged.

There were likewise one or two automatic telephone switches which provide for subscribers to a small telephone system obtaining connection with other subscribers without the intervention of a switching operator. So far these systems have a limited use where a service designed to provide for special conditions is required.

A neat little switch-board for private use was shown by the "Société par Actions du Bureau Electrique," in which connection between any two lines is effected by pressing a button which makes the necessary line contacts and remains depressed until on the receipt of the ring-off signal it is released by giving it a quarter revolution on its axis. It then springs back automatically into the position of rest and restores everything to the normal condition. The use of pegs and cords is thereby dispensed with.

Carbons for Microphones.—The manufacture of carbon granules for microphones has received considerable attention. These appear in two forms—one the irregular granule that so far has been most generally used in this country, the other consisting of small spheres varying in size from ordinary dust-shot to the $\frac{1}{16}$th of an inch in diameter. There appears to be little to choose between the two forms of granules for microphone purposes.

I observed great diversity in the details of construction of telephones and microphones exhibited by French firms. The State Administration compels subscribers to provide their own telephones, and these may be purchased at will from any one of a number of manufacturers who comply with the specification of the Administration. The result is that a very large number of local manufacturers have sprung up throughout the country, each exhibiting a special telephone which differs in some local details from all others. The result, from an exchange point of view, is not good. The speech from different subscribers varies materially according to the type of instrument used, and none of these local inventions are better than the ordinary type of telephone in use in this country.

As illustrating various modern methods of laying underground conduits for telephone and power circuits, the cement duct system designed by Mr. Hultman, of Stockholm, was exhibited in the Swedish section, and the Belgian Administration showed in their State Pavilion a section of

the single-duct built-up system which they have laid in Brussels. Both these methods are in use in this country, the Hultman system having been very largely used by the National Telephone Company, whilst the single-duct built-up method has been adopted by the Post Office for its London telephone system. The Hultman system is perhaps the cheaper of the two, whilst on the other hand the built-up system is, I consider, the stronger and more reliable. A few blue-prints illustrating the method of construction used in London are on the table, and can be examined at the close of the paper.

Wireless Telegraphy.

There are four exhibits of wireless telegraphy in the exhibition.

Post Office System.—An illustration by means of coils of the Post Office Electro-magnetic method originated by Sir William Preece appears in the Post Office exhibit.

Ducretet and Popoff.—Monsieur Ducretet exhibits his Hertzian system, which it is understood he designed in conjunction with Monsieur Popoff, the Russian inventor, and which has been adopted in the French Navy and, it is believed, in the Dutch. There was nothing to specially distinguish it from the Marconi system in this country.

Slaby and D'Arco.—The Algemeine Elektricitäts Gesellschaft exhibits a system designed by Slaby and D'Arco in Berlin, and this deserves a little consideration, as the inventors have replaced the usual vertical insulated wire, so well known in connection with wireless systems in this country by a vertical cage, the upper end of which is connected to earth. This is illustrated in Fig. 6. At first sight it would appear as if the effect of the cage would to some extent be neutralised by the waves emanating from the return wire to earth, but probably the self-induction of the earth wire practically confines the oscillations to the cage. This, however, is an interesting subject for speculation and investigation.

The American Wireless Telegraph Company exhibits a system in which a pneumatic arrangement has been designed for decohering, the object being to reduce the number of relays and magnets in the combination, with a consequent

diminution of the local Hertzian waves which tend to interfere with accurate reception.

Poulsen Microphonograph.—Perhaps the invention of the greatest scientific interest is the Poulsen Microphonograph, by which a telephone conversation can be permanently recorded on a steel wire, and reproduced at any time.

In this apparatus a steel wire, or a steel band, is moved by any suitable means with considerable velocity between the poles of a small electro-magnet. On speaking into a telephone transmitter joined on the circuit, the undulatory

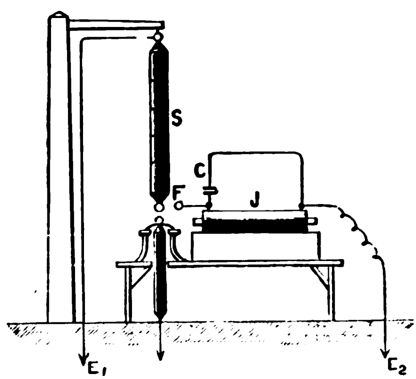

FIG. 6.

currents set up in the transmitter react upon the electro-magnet and cause a continuous variation in the direction and in the degree of magnetism at the poles of the electro-magnet. These variations are permanently recorded on the steel wire as it rushes by, and when the message is complete the steel wire retains a definite record of what has taken place in the shape of a continuous series of transverse magnetised lines varying throughout in their polarity and in their strength. On connecting a telephone receiver to the electro-magnet, and again starting the wire on its course, this magnetised wire generates electric currents in the coils of the superimposed magnet as it passes between its poles, and these electric currents, which are the exact counterpart of those generated by the original voice, cause the telephone to repeat what was said in an almost absolutely perfect manner. In one variation of the instrument an endless

steel band was caused to revolve at a high speed around two wheels which stretched it out to its full extent. On one portion of the band was placed a magnet connected with a microphone; further on were half a dozen electro-magnets connected with as many telephones; and finally an electro-magnet through which circulated a permanent current. As the band rushed by in the course of its revolutions it picked up the magnetism from the speaking microphone circuit, next it reacted on the electro-magnets connected to the telephones and caused them to speak, and, finally, on passing under the electro-magnet through which a steady current was flowing, the whole of the impressed magnetism was neutralised and the band wiped clean, so to speak, and rendered ready to receive a fresh impression.

At present this invention is in the early stage of scientific discovery. It may be used by a telephone subscriber to record an important communication, and it promises to afford means of obtaining a telephone repeater, a problem which has been before the electrical world for the last twelve years, and which so far has not been solved in a satisfactory manner. A telephone repeater would increase the range of telephonic speech and decrease the cost of long lines. The President of one of the American telephone companies some time ago offered publicly a reward of 1,000,000 dollars for a thoroughly satisfactory telephone repeater, but the money has not yet been earned.

Telephone Hirmondo.—Amongst the miscellaneous exhibits is one by the "Telephone Hirmondo" which provides news and musical transmission alone, but without telephone intercommunication, in the city of Buda Pesth. Technically there is very little information to be derived from the exhibit, which is rather statistical than technical, but I was rather surprised to learn that the number of subscribers to the system had increased from 3,750 on the 1st of January, 1895, to 7,560 on the 1st of January, 1900, all the wires being overhead. In practice items of news are spoken into the main transmitting telephones at the central station every quarter of an hour, and such items are repeated until a fresh batch is started. So far it is an electrical substitute for an evening paper combined with a theatrophone installation.

I came across a novelty in telephone administration. In

the capital of Mexico they have absolutely free trade in telephone exchanges, which may be erected by any body or company with the sole proviso that there must be intercommunication between the systems. There are eight separate companies at work, with an aggregate of 4,000 subscribers, the average rate of payment being 12½ francs per subscriber per month.

At the meeting of the Electrical Congress various papers were read which have been or will be published. A committee was likewise formed to consider the question of units, and some radical proposals were brought forward, which would have involved an entire change in the existing methods of electrical measurement. The whole subject was very keenly debated, and anything in the nature of a radical alteration was outvoted. The committee restricted itself to giving definite names to two of the existing units, which up to the present had been known as the "C.G.S. unit of magnetic field," and the "C.G.S. unit of magnetic flux." These propositions were then submitted to a meeting of the Official Delegates, and were practically carried unanimously. A further proposition was submitted to the delegates to the effect that electrical energy was property, and that its theft should be punishable by law. This was also carried. In Great Britain existing Acts of Parliament define electrical energy and provide against its theft, but in certain countries it has been argued that electricity cannot be weighed, and that no punishment can be inflicted for stealing it, hence the above resolution.

In the foregoing curtailed review it is of course impossible even to refer to many of the excellent exhibits of working apparatus, line stores, cables, both submarine and subterranean, and other modern and up-to-date telegraphic and telephonic materials and appliances that were to be seen at the Exhibition. If I have had occasion to omit even a casual reference to many beautiful and elaborate exhibits, this is not due to want of appreciation, but to the fact that to do justice to all would involve a complete treatise on telegraphs and telephones. It is hoped that the few remarks I have made on the most striking developments will be of sufficient interest to have justified the presentation of this paper.

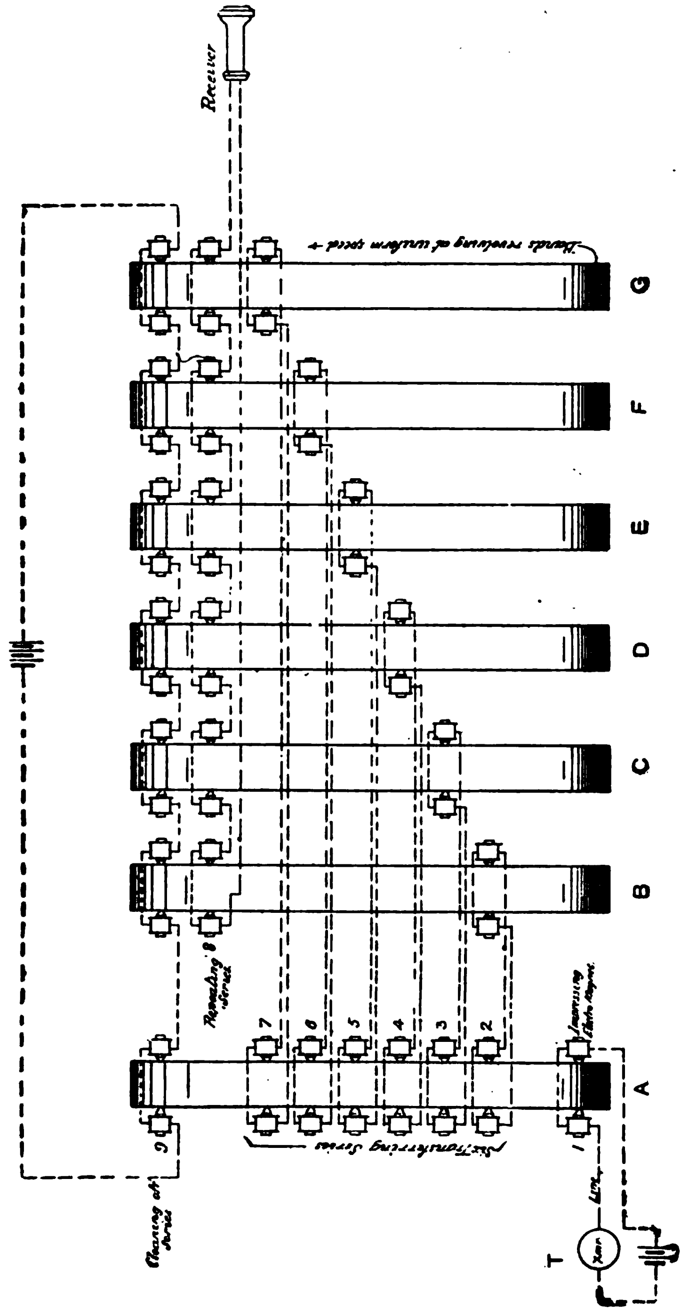

FIG. 7. (See Addendum, p. 92.)

ADDENDUM.

The author in the course of his paper sketched on the blackboard the figure given on p. 91, and added the following explanation with regard to the possibility of applying the Poulsen microphone to the development of a telephonic repeater :—

As illustrating a method which it is thought might be developed into a satisfactory telephone repeater, the following was suggested as a groundwork to be built upon. Assuming, as illustrated in the figure, that there are six or more endless steel bands, A, B, C, D, &c., revolving at an absolutely uniform rate around suitable pulley wheels, and that, approaching closely to the band A are the poles of an electromagnet 1 connected to a telephone T; that a little further on an electromagnet 2 is mounted in a similar manner, but connected to a corresponding magnet on band B, and beyond this an electromagnet 3 establishes a relation between bands A and C, and so on, next that bands B, C, D, E, F are fitted with a number of electromagnets, 8, joined up in series and connected to the line over which it is desired to transmit, and finally a series of electromagnets through which a permanent current circulates are fixed at point 9.

Then, on starting the apparatus, and speaking into the telephone T, band A is energised by the series of transverse magnetic polarities due to the variable currents in the electromagnet. As the band in its revolution passes under magnet 2 it generates currents which are transmitted to its corresponding electromagnet on band B, there to repeat the magnetic effect. The same action takes place between electromagnet 3 and band C, and so on until the whole of the independent bands are each impressed with the same magnetic alphabet. On completing half of the revolution the bands B, C, D, E, F pass under and energise the series of electromagnets which are joined up in such a manner as to combine the electromotive forces due to each separate band, and lastly the series of electromagnets 9 wipes out the whole record, and leaves the bands free to be acted on by fre[illegible]ons.

Sir Wm. Preece.

Sir WILLIAM PREECE: It does my heart good to see such a crowded assembly to listen to an address which has telegraphy principally for its text. And looking round this room, I cannot help going back to the early stages of the history of this Institution, when sensation after sensation was brought before us, and the room itself was filled to overflowing, and applause as ringing and as cheerful as that which you gave Mr. Gavey at the cessation of his paper was the order of the day. I wonder if young members of this Institution ever take the trouble to read the early volumes of the Proceedings. They will find there a good deal that is interesting; they will find there a good deal to learn, and a great many facts which would save them from many troubles. It is not the first time that we have heard a paper in this room describing the exhibits as shown at different Exhibitions. I have had the gratification myself on more than one occasion of bringing before the Institution things shown in Paris, in Chicago, in Philadelphia, in Münich, and in Vienna, and I hope and trust that the practice originated in the very early days of the Institution will be continued, and that we shall take every possible opportunity to find out what progress is being made in other countries, and to show that we are not behind. If we do not look out, however, we shall get behind.

It is a remarkable fact that, in nearly all branches of progress the growth of business varies almost exactly with the growth of the capacity for carrying that business. Mr. Gavey has brought before us several novelties shown in Paris—Rowland's Multiplex, Mercadier's Multiplex, Virag and Pollak's system, and other improvements; but this axiom, or maxim if you like to call it so, that the growth of business varies with the capacity for conducting that business, is not confined to telegraphy: it is true of telephony, it is true of electric traction and electric railways: it is true for every branch of the applications of electrical industries. We never have had it more strikingly shown than in the fact that in England and in America, and wherever telegraphy has progressed, while the rate of increase of length of wire grows in a straight line, or in arithmetical ratio, the rate at which the business increases is a rapid ascendant curve; and it shows that, while we are able to compete with that business and perform the work as fast as ever it was done before, and sometimes faster, the invention of the electrical engineer is exceeding the rate of the growth of the erection of the wires, so that, at the present day, a circuit will carry quite ten times as much as it carried when this Institution first commenced its meetings in this room. That is a great fact, and Mr. Gavey has well brought it before us. He has told us a great deal about telephones. Well, that is a large subject. The telephone was first shown in this country, I believe, in Plymouth, and next in this room, and a crowded meeting assembled here to see this wonderful instrument for the first time. Twenty-three years have elapsed since that period, and where are we now? Is the telephone industry in this country in a flourishing or in a proper position? We see a kind of triangular duel going on. We have on the one hand a company striving hard, doing its best to maintain a very difficult position; we have municipalities springing up trying to emulate their would-be competitor; and we have in the background a small depart-

Sir Wm. Preece.

ment called the Post Office Department, that does its very utmost to meet the wants of the public, and has this great advantage over every other one of its competitors, that its back is broad enough to bear the greatest possible abuse which can be brought against it. I envy the position of Mexico. There, Mr. Gavey says, everybody may start his own exchange, and be placed in communication where he likes and with whom he pleases, on condition that they are to have free inter-communication. But how are they to have free inter-communication? for every inventor has his own switch, his own transmitter, and his own instrument? The difficulty is bad enough as it is, with three competitors. Mr. Gavey, I think, said there were six in Mexico, and probably that will grow in the same compound ratio. And when those who have been to Mexico next go there, perhaps the six may become sixty times six, and then where are you? If there is a business in this world that should be manipulated and managed by one administration, and by one concern, that should be conducted exactly on one principle, it is the business of telephony. I did hope that I should see a day when every house in this country should have its telephone, and that there would be a practical and easy mode of placing every house in communication with every other house in the whole of the United Kingdom. That is thrown to the winds with a great many other wild illusions and wild imaginations. It may come in the distant future, but I am afraid that, with grey hairs coming so rapidly, I shall never see my ideal telephone system carried out.

Mr. Gavey told us something about the exhibits of wireless telegraphy. We are getting tired of wireless telegraphy. The papers are still continuing to take it up wildly, and to advertise strange experiments in different places, but we want to see practical wireless telegraphy—and where do you find it? Is there at the present moment anywhere a single circuit worked commercially on a practical system of wireless telegraphy? If there is, let somebody come and read a paper before this Institution. The only place is the Post Office. The most beautiful thing, the greatest novelty that Mr. Gavey has brought before us, is this Microphonograph of Mr. Poulsen's. Apparently there is very great difficulty in showing it in operation. I could not see it in Paris; I tried very hard, but I could not see it there; it had been sent to Berlin. But it is coming, and it is a very marvellous thing. It is not only in itself, as the description has shown us, beautifully designed, based on beautiful principles, but it is one of those things that is going to open the eyes of all our physicists, scientists. and theoretical men, on the question of the molecular character of magnetic and electric operations. There is nothing, to my mind, so marvellous as the continual exchange through electrical actions and magnetical actions, through steel, and through circuits of these curious changes of position, form, motion and shape that we sometimes call electrical and sometimes magnetical, but are all brought out in this beautiful instrument of Mr. Poulsen's.

Lastly, Mr. Gavey referred to the work done by the Congress. He mentioned the fact that the Congress had decided upon introducing or recommending the adoption of two units, (1) the "C.G.S. unit of Mag-

ment Field," and the other "C.G.S. Unit of Magnetic Flux." But he did not mention that the name accorded by the Congress to the unit field was the name of Gauss, and that that accorded to the unit flux was that of Maxwell. I have serious doubts whether the name Maxwell will come into practical use. Probably the abbreviation for the word "Max" may come into use, but I have little doubt that those who talk and who are our writers may probably take up the unit of field and call it the Gauss, and that that will rapidly come into general use. Gentlemen, I am proud to see my successor in the technical chair at the Post Office bring a subject before you with the clearness that Mr. Gavey has to-night, and I hope and trust it is only one of an annual display on his part of the progress made in a branch of electricity [illegible] that is thought of too little; and I am sure that the old telegraphic faces that I see here to-night will be tempted perhaps to come again and see for themselves, or hear for themselves, that other branches of electricity are as interesting to them as telegraphy and telephony. Sir W. Preece.

The PRESIDENT: Of all our visitors from America I believe Mr. Hammer alone remains. We should be very pleased to hear any remarks from him. The President.

Mr. W. J. HAMMER: I have had the pleasure, since I visited the Electrical Congress at Paris, of taking a trip around the Continent, and I have seen a great many very interesting things, both electrical and mechanical. But I am scarcely prepared to stand before an audience of this character and speak without some preparation or in a superficial way of many of the things which I have seen. One of them has been referred to here to-night, but I think it has been given a wrong title. The Telephonograph, I believe, is the correct title of the instrument. I have recently been in touch with some of the people who are identified with that instrument, and they have expressed themselves strongly as desiring to have that name used. I saw in Berlin some very interesting modifications of that instrument, and some very interesting experiments, but I hesitate about speaking on them because they were shown to me under rather peculiar circumstances, and in view of certain patents I do not think it would be well to refer to them here: but I can heartily endorse what Sir William Preece and Mr. Gavey have said about the remarkable interest which this instrument has aroused at the Paris Exhibition, and of its beautiful simplicity and perfection. I have had considerable experience in connection with phonographs, and working with them, and have tried most of the different modifications of phonographs that have been made, and I certainly never have heard anything in the way of a speaking machine that will touch the telephonograph. With every instrument in which the sound-waves are recorded by something in the way of a stylus, there is bound to be a noise which cannot be eliminated, but where these lines of force are stored up in the wire in the silent manner in which they are, the most perfect reproduction is produced. In Paris and also in Berlin I made some experiments in using words, the recording and reproduction of which is very difficult in a phonograph, also in breathing and whispering and that kind of thing, but all of these are perfectly reproduced by the telephonograph. So that Mr. Hammer.

Mr. Hammer

I feel very certain in my own mind that it is superior to anything that has ever been made in the way of a talking machine.

I had the pleasure of seeing a little of the Nernst lamp whilst I was in Paris, and in following it up somewhat on the Continent, but that is a subject that does not really come within the province of the discussion of this admirable paper that Mr. Gavey has brought before you; in fact, quite a number of the subjects that I have looked into and studied during my sojourn in Europe, have no direct bearing on the paper, so I hesitate to mention them. But there is one thing which Mr. Gavey brought before you that I had the pleasure of seeing in Buda Pesth, and that is the Virag and Pollak system of telegraphy. I have some samples of the writing, and I have seen the apparatus in operation. It is certainly in its present form a most interesting and promising invention, and seems to work in a most satisfactory manner.

Mr. Roberts.

Mr. M. F. ROBERTS: Mr. Gavey has told us of the difficulty he has had in explaining the operation of one of the instruments owing to the request that was made to him not to reproduce the diagrams. A similar wish on the part of many of the exhibitors was one of the great difficulties I met with in studying the different exhibits in the class of Telegraphs and Telephones in the Paris Exhibition. I found that, with one or two notable exceptions, assistants were in charge of the exhibits who either had not the necessary knowledge to explain them, or who were definitely instructed not to do so. The difficulty was added to by the want of a suitable catalogue. I have been told by different gentlemen who sent workmen specially over to the Exhibition, that the visits in many cases were of little use, simply because the representatives they sent could not obtain the information they wished them to get. In the case of telephones and telegraphs, I think we may say generally that, with one or two exceptions, perhaps, which Mr. Gavey has pointedly brought before us, the improvements are to be found in points of detail. I should have been particularly interested if Mr. Gavey had extended his remarks with reference to the progress made between different Exhibitions by briefly referring to what had been the most marked improvements since the Exhibition of 1878. To my mind it appeared that, in the case of telegraphs, the improvements very largely consisted of higher finish in the apparatus exhibited, whereas in telephony the improvements appeared to be chiefly in the simplification of parts and the design of parts which can be readily produced at cheap rates. Of course we can understand the development in the case of telephony taking this line, because there is such an enormous multiplication and reproduction of the same part, whereas in telegraphy the problem that has to be dealt with varies considerably in almost every case. Not only, too, does it vary in the case of a given line but it may perhaps vary markedly at different times of the day. The successful design of parts for telephone-working appears to be largely due to the specialising of certain forms. For example, in the case of one of the exhibitors to whom Mr. Gavey has referred, I found that the firm had one gentleman present who had made a special study of switch-boards, another who had made a special study of telephone instruments, and a third who told me his department was to give in-

formation respecting dynamos. We can easily understand, when a powerful firm has representatives and employés who devote their attention almost exclusively to one part, that we get these improvements which Mr. Gavey called attention to in the case of the central battery system of working, and it is this specialising in America which has resulted in the very great progress which was noticeable in the American exhibits in the Paris Exhibition. Mr. Gavey has also called our attention to the Rowland's telegraph apparatus. If we go back to former Paris Exhibitions, in every case I think we have had type-writing instruments, but it is an astonishing fact that type-writing machines have made such little headway in this country. Nothing is more to be desired than a type-written message, but yet at the present time such a message is practically unknown in this country. I do hope that the very successful exhibit of the Rowland's Company will have the effect of popularising the use of the type-writing telegraph here. Mr. Roberts.

Mr. A. W. HEAVISIDE [*communicated*]: Whilst thanking Mr. Gavey for his brief review in popular language of the Exhibits, Class 26, of the Paris Exhibition of 1900, one is tempted to ask for more, the motive being that subjects so deeply interesting as electrical application in any form, need much detail to be of practical utility. Mr. Heaviside.

So far as I know, though much experience has been gained in the use of the Delaney multiplex system, the valuable electrical data relating thereto has not been published to the world. For instance, how useful it would be, if the profession were fully acquainted with actual time-lag under practical conditions with given conductors, both copper and iron, with an impressed force of a given value, the effect of leakage at the insulators, the practical difficulties experienced in a variable climate like ours, with rapid alterations of heat and humidity, frost, snow, and sleet and rain in twenty-four hours.

Similarly, in Rowland's multiplex, one is tempted to ask, does abrupt omission of two half waves produce any deleterious re-action from the electro-static discharge. The operator's control of the position of the receiving roll by means of a transverse mark appears to be weak. This roll, if feasible, should be automatically controlled. The advantage that the messages are delivered from the receiving instrument in a condition to be sent out for delivery, is no mean one, if it really saves a corresponding number of receiving operators.

Mercadier's system of super-imposed vibrations at the sending end, and selective apparatus at the receiving end, is most suggestive. If twenty-four super-imposed vibrations can be sent simultaneously, and each receiving apparatus can be tuned to respond only to that which belongs to it, then why not have a main pair of conductors of considerable cross-section with twenty-four loops of variable length to twenty-four different stations?—what a saving of wire and unburdening of the telegraph lines, which groan under their ever increasing load, this would lead to. Of course, metallic loops must be used, and this speculation revives a 19-year-old suggestion of mine for working the Wheatstone automatic and is probably worth trial, as anything that has for its object a reduction in the number of wires and stability of signalling is worthy of much thought.

Mr. Heaviside.

The tendency of the age is to work all electrical currents in metallic loops, which surely will become increasingly necessary as electric light and power applications are developed. Our mother earth is impartial, she treats all her children alike, her ample bosom receives them all, they lose individuality, which is the very essence of the telephone and will become a necessity with most other apparatus—though all the work of transmission and signalling is done in the environment, after all, the guides, in our present state of knowledge, in spite of Marconi's practical application of the labours of Righi, Branly, Lodge, and others, cannot be dispensed with. Virag and Pollak's photo-autographic telegraph apparatus is marvellously clever, but apparently wanting in simplicity at present. That it may become commercially applicable every one desires, and to throw cold water upon it would be as bad as calling the telephone a philosophical toy!

But, may I venture to suggest in this, and in all other systems described by Mr. Gavey, how useful it would be at least to append to his paper a theoretical diagram illustrating the principle of each case. It must be remembered that there are many students of the Institution, and life is too short for every individual member to look up all the authorities. It is a case of "Science Abstracts" over again. One at least wants to know the text, and one can draw the moral in solitary railway journeys and other fugitive snatches of time for thought that offer in these busy executive days.

Central battery telephone switches are on their trial, and if the extensions from the simple subscriber's circuit from his telephone to the central exchange can be adapted to the many diverse needs the telephone has to meet, it will probably have come to stay. It is hoped that the new decade will see such a development of the telephone in Great Britain and Ireland as shall remove the reproach that we now suffer from of being behindhand as compared with other countries. One feature of the central battery system, if I rightly understand it, is that it secures privacy of communication between the users, a feature that has distinguished the Newcastle-upon-Tyne exchange for many years and commends itself to all.

Then as to carbons for microphones, much is still wanting in microphones; how to get loud speaking without jarring, has been a problem since the days of Hughes, and it still remains a problem. An ideal microphone should respond in indentically the same manner from day to day to every vibration with great delicacy, and invariably recover itself without lag or jar. Take a curl of horsehair as used in domestic furniture. In general, it yields and recovers from any impressed force with precision, its elasticity being so great and its fatigue so small; and if carbon filaments could only be manufactured with similar qualities and arranged in companionship like the horsehair composed of many filaments, an ideal microphone might result. I have experimented with carbon filaments and have found the most beautiful speech result, but their brittleness brought with it a short life. Perhaps makers of incandescent lamp carbons could solve the problem. Would that we knew of all the failures and could thus avoid much trodden ground!

Of rophonograph one can only admire and wonder, what

Mr. Dane Sinclair.

Mr. Dane Sinclair: I do not know that there is much to be said on Mr. Gavey's excellent and clear paper. He has described to us what he saw at the Exhibition. The improvements in telegraphy, when summed up together, are in themselves as beautiful as they are marvellous. The improvements in other directions, and especially in telephony, he has referred to graphically, and, I think, accurately. I am not sure whether he may not be too enthusiastic on the common battery or central energy system; it is just possible, but time will show. When Mr. Gavey spoke of the latest invention in telephony connected with the phonograph he called it the microphonograph, but another gentleman who has spoken prefers to call it the telephonograph. I think the latter is perhaps the more accurate description, but in justice to Mr. Gavey I may say that when I was at the Exhibition the name which he has given to it was the name by which it was generally known. In connection with this I am glad to say that for once in my life I have been ahead of my good friend, Sir William Preece. When I was at the Exhibition I had this thing placed before me and set to work under all the conditions that I could possibly wish for. To me it seemed marvellously pretty and wonderful. There is no doubt, as Mr. Gavey and another speaker have already said, that it gives us a distinct record of speech, so distinct that it cannot be compared with any record we have had hitherto. A long steel wire is wound helically on a drum, the revolution of the drum causing the wire to pass in front of and against the attenuated poles of a receiver magnet. By this means the variations of magnetism caused by the currents from a transmitter in the circuit are magnetically recorded on the steel wire, so that when the latter is again driven past the receiver poles the original speech is reproduced in an ordinary telephone receiver absolutely pure and without any of the grating sounds that come from the phonograph. If this invention does give, as Mr. Gavey hints it may, a telephonic relay, then it passes at once out of the range of purely scientific interest into that of commercial value. Under Sir William Preece's directions very heavy copper wires have been erected from one end of this island to the other. If we can have a telephonic relay, these heavy copper wires of 800 lbs. to the mile become no longer a necessity. They are good in themselves, but if science can teach us how to make a telephonic relay possible, large copper conductors will become things of the past. Of course the advantage to users will be that as the price of the conductors goes down, they will in time get the benefit of that reduction. I was very much impressed by this invention, and believe with Mr. Gavey that there are large possibilities in its future, and I was rather disappointed to see that it was not spoken of by the electrical press in London in a way that I think it deserved.

Mr. Kingsbury.

Mr. J. E. Kingsbury: With regard to that instrument, which does not seem yet to have been effectively christened, I confess when I saw it I thought that whilst we gave credit to Professor Bell for a very bold idea in expecting to put on to an electrical wire electrical undulations representing sound-waves, I thought that to go just this step further and magnetically record those undulations on a wire was an idea hardly less bold. As a scientific instrument, as Mr. Gavey says, it is of the

Mr. Kingsbury.

greatest interest. Of the exhibits at Paris we are bound to give it the first place, and to regard it as a grand application of a very clever idea. The practical uses which may lie in store for the instrument it is, of course, a little too soon for us to remark upon. I would like to say one word in reference to Mr. Gavey's observations on the application which the Post Office have made of automatic signalling. The Post Office introduced in the first instance a telephone system which enabled a signal to be sent automatically, requiring no exertion on the part of the subscriber, sending a signal by the natural operation of taking the telephone off the hook. The Post Office adhered to that system absolutely, much to the surprise of some people who thought, perhaps, it had not all the good features that it should have. It had, at least, that important feature of automatic signalling, to which attention is necessarily drawn now, and which forms one of the features of the central battery system referred to by Mr. Gavey. That system is not only an application of one important feature such as automatic signalling, but such features as the centralisation of the battery and the energy required to work the system. Nor is it a system which has been developed in a day. Twenty years ago a patent for the centralisation of transmitter batteries was taken out by Mr. Scribner, to whom, perhaps, the largest credit is due for the later developments. Mr. Heaviside, in his communication, thinks that the system may live when it is adapted to all the circumstances of the telephone service. I think it may be said that there are no conditions of the telephone service known to practical telephonists which the central battery system, described and shown at the Paris Exhibition, does not meet. Mr. Sinclair is of opinion that Mr. Gavey is a little too sanguine. I think Mr. Sinclair ought to have expressed those views a little more strongly, and given some reasons for them. My impression is that, as a member of the Jury of the Paris Exhibition and in some other capacities, Mr. Gavey has made a sufficiently thorough study of that system to express an opinion. It may be that Mr. Sinclair will agree later on, when time has still more shown the satisfactory working of the system.

One point with regard to the telephonic repeater. I would remind Mr. Gavey that, whilst, perhaps, the question may have been raised more specifically twelve years ago, still it is of earlier origin, for Gilliland took out a patent for a telephonic repeater in 1879. About that time there was, so to speak, an epidemic of telephone repeaters, although there was no million-dollar-reward for them. It is a curious thing that when there is no large reward offered there seems to be a prolific supply, and now there is a large reward I do not know of anything in the nature of a practical repeater put forward. The first telephonic repeater does, to my mind, seem to have the germ of a working instrument, even more so than the beautiful instrument we have had described here. The idea was to have a telephonic diaphragm as a receiving instrument, and make it a combination instrument, attaching it to a microphone transmitter. Although the original in[illegible] has been before the public for twenty years, it has not bee[illegible] any practical stage.

Professor Silvanus Thompson: Three points in Mr. Gavey's most interesting paper seem worthy of additional remark. The new magnetic units with which the Paris Congress has endowed us are certainly a great boon to us in any case, speaking about magnetic quantities. We are able now to say definitely that where the earth's magnetic field is to be described as having a strength of 0.18, it is not 0.18 hung up in the air with nothing to it; it is 0.18 gauss. We have all sorts of magnetic fields which can be easily described as to their intensity, up to the strong magnetic field in the air-gap between the poles and the armature of a dynamo, which may run up to be as strong as 6,000 or 7,000 gausses. It is a great gain to have a word to tack on to the idea. I am not quite so sure that the other name, maxwell, is destined to receive the same immediate use as the word gauss, because it is merely a synonym for that for which we had a shorter name before, viz., a magnetic line, and it is just as easy to talk about one million lines as one million maxwells; indeed, it is rather shorter to say a "megaline" than a "mega-maxwell." I wish they had given the name maxwell to the product 10^8 magnetic lines, so that we could have had the one *maxwell* as the name for that amount of magnetic flux which if "cut" in one second would produce an induction of one volt. Prof. Thompson

I had an opportunity of inspecting the method of wireless telegraphy devised by Professors Slaby and Count Arco, not only in the Paris Exhibition, but also, previously, in Berlin, and to witness its working from Berlin to Oberschönweide—I do not know how many miles; some seven or eight miles, at any rate. I do not think that the diagram in the paper, however excellent it may be, would quite convey to anybody who had not seen Arco's arrangement exactly what it was. The vertical cylinder in Fig. 6 represents the wire cage—if you can call it a cage—made of either six or eight wires, I forget which, quite ordinary thin wires about 100 or 150 feet long, joined to a ring about six feet across at the top and another ring six feet across at the bottom, and slung from a tall chimney, or something of that kind. It is very remarkable indeed that you should be able to send wireless messages from one of these cages to the other when the tops of the cages as well as their bottoms are earthed. The one thing on which emphasis has hitherto been laid in wireless telegraphy, so-called, was that you should have an elevated and insulated conductor. Well, this conductor may be elevated, but it is certainly not insulated, and nevertheless signals are transmitted with absolute certainty.

Lastly, I also would like to bear testimony to the extraordinary perfection of articulation, of that recording and speaking phonograph and telephone which I had the opportunity of seeing in Paris. It was not then called either the microphonograph, the microtelephone, or even the telephonograph; it was called the *telegraphon.*

Mr. J. Gavey [*in reply*]: My reply on the discussion need not be lengthy. Mr. Roberts referred to the desirability of pointing out more definitely the improvements which have taken place in telegraphy and telephony between the year 1878 and the present date. Now, in preparing this paper, I inclined to make it suggestive rather than to Mr. Gavey

Mr Gavey. state too definitely what I thought the improvements were. But in answer to the remark I should like to say this, that although between the year 1878 and the present time improvements in telegraphy, except in details, have not been marked, I certainly think that combining what we have seen at the Paris Exhibition with other inventions which did not appear amongst the exhibits, but which are coming to the front, we may expect very marked improvements in our methods of telegraphy in the immediate future. What I mean is this. At the present time we have in England and America our good old servant the Morse system, which has rendered invaluable service throughout the whole of the world, but which I cannot help thinking is not quite up to our modern requirements. I am looking forward to an improvement in the direction of transmitting messages by a simple method, such as a type-writing key, which will either transmit direct, as in the Rowland's, or which will perforate slips, as in methods designed by a couple of other inventors, slips that will be passed through an automatic transmitter at a high speed, and that will admit either of the typing or of the writing out in cursive characters the messages at the far end without the necessity for transcribing, and without, let me say, the somewhat messy operation of sticking down Hughes' slips on a sheet of paper. In making that remark, I wish it to be understood that I do not in the slightest degree wish to undervalue the lovely instrument that was designed by Professor Hughes, and that has been so extensively used on the Continent; but I think every one will admit that an instrument which will either print or write, I do not care which, on a column of paper a message in such a form that it can be immediately sent out to the recipient, is far and away in advance of anything we have yet had. I venture to prophesy that we are within a measurable distance of introducing and using instruments of this type. With reference to telephones, Mr. Roberts' remark is a trite and a true one; that is to say, the improvements have been mostly in detail. And, after all, there is no other improvement possible. For a telephone you must have an instrument that you can speak into and hear by. You want a line, and you want switching apparatus which will meet the conditions of switching one subscriber through to another readily. Given these, which are absolutely necessary, all other improvements must be improvements in detail, and I venture to think that the improvements in detail which have been made in telephony within the last four or five years have amounted almost to a revolution. With reference to another criticism that a more extended description might be given in the present paper, as I said before, the paper is rather suggestive than descriptive.

Professor Thompson has referred to the cylinder in Fig. 6. I am not responsible for the engraver, but that was copied almost exactly from a pamphlet that was given to me by the exhibitors at the Paris Exhibition, and it was supposed to describe accurately Slaby's and D'Arco's system. In conclusion I wish to return thanks both on behalf of the Institution and myself, to the proprietors and editors of *The Electrician* and *The* [illegible], attaching the loan of all the blocks that are used in illus- [illegible]

T. [illegible] Gentlemen, this is the first meeting of the session.

The President.

We have had a most excellent paper, and it has led to a very fine discussion. I propose a vote of thanks to Mr. Gavey for his paper.

The vote of thanks was carried by acclamation.

The PRESIDENT: I will now call upon Mr. Goolden to describe and to demonstrate the use of the Stelje's Type-writing Telegraph.

THE STELJE'S TYPE-WRITING TELEGRAPH.

Mr. W. T. GOOLDEN: The instrument which I have the honour to bring before the notice of this meeting, although it will not have the scientific interest and importance of those which have been described by Mr. Gavey in his paper, will, I think, have some interest for the members. It is the outcome of a very considerable number of years' work. Ever since the first introduction of the Wheatstone A B C telegraph, a large number of people have tried to produce a printed record of messages sent. This instrument, which has been invented during the last few years, has achieved that object, and at both ends of the wire a record of the messages sent is obtained, preventing mistakes which are sometimes made by the original A B C indicator, and also by the telephone, and preventing any dispute as to the actual message which has been sent. The principle underlying the success of this instrument is, I think, the inversion of the method usually adopted. Instead of the current which is sent along the line doing the work of printing, the current merely controls. The mechanism of the work is done by weights or springs. There are two trains of wheels, one actuating an escapement which brings the type-wheel into position, and the other train of wheels which does the actual printing by means of a lever. The printing lever is held up in rather an unusual way by the alternate currents against an electro-magnet, and when the current is stopped the lever allows the printing to take place by means of the weights or springs. A point of interest that I should also like to bring before you, in connection with this, is that this is particularly applicable to telephone lines. We have had a successful trial of this instrument, printing between London and Manchester on a loop line, and we have also found that we can use the instrument while the telephone is actually working without any detriment at all to the neighbouring wires and without any noise on the telephone. Those points you will be able to see for yourselves outside. There is an instrument downstairs in the hall and another one at the top of the stairs to which telephones are attached, and you will see that by means of choke-coils which are placed in the circuit, the two instruments, the telephone and the printing telegraph, can be worked simultaneously without any detriment the one to the other. There is another instrument also outside, the invention of Mr. Higgins, which I do not yet know very much about, so I cannot describe it in great detail, but it appears to work extremely well. You will see it for yourselves.

The PRESIDENT: I will ask you to pass a vote of thanks to Mr. Goolden for this description and also for the demonstration he is about to give downstairs.

The vote was carried by acclamation.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected :—

Members:

James Franklin.
William Henry Trentham.
Robert Robertson, B.Sc. (Edin.), M.Inst.C.E.

Associate Members:

Leonard James Aron.
Marshall Handyside Bennett.
Robert Beveridge.
John Bowden.
George Dalton Dauncey.
John Lambert.
John Todd Morrison.
Wilfred Ernest Pennefather.
Henry Nicol Thomas.

Foreign Members:

Professor Riccardo Arnó.
Professor Mikail de Châtelain.
Guido Semenza.
William Gordon Thomsen.

Associates:

William James Baker.
George Ernest Cummings.
Arthur Joseph Drakes.
Edward George Fenning.
Wilfred Gaye.
James Olliff Griffits Gibbons.
George Gordon.
Isaac Bridge Hadaway.
Edwin Percy Harvey.
Adam Henry.
Charles Frederick Maxwell Hibberd.
Frank Holland.
Reginald Herbert Kent.
George Kelly Nowlan.
Davidge Page.
William Harry Ridpath.
Frank Harold Rippon.
Edward Lyle Rossiter.
Albert R. Turner.
Harold Wragg.

Students:

Earnest Rosling Alexander.
Denis Robert Howe Browne.
Oswald Henry Browne.
Harry Leslie Churton.
Reginald Cecil Creasey.
Edgar de Lautour.
Charles Henry Fisher.
Robert Benjamin Forster.
Richard Walter Gregory.
Sydney Hubert Harris.
Arthur Lawrence Kavanagh.
Robert Martin Longman.
Frederick Oldham Mills.
Leopold Romero.
Henry William Taylor.
William Bernard Thompson.

GLASGOW LOCAL SECTION.

Paper read at Meeting of Section, November 14th, 1900.

ELECTRICITY SUPPLY.

By W. A. CHAMEN, Member.

This subject has been dealt with so often and by such able hands that the author feels some diffidence in venturing to write a paper upon it. The brief remarks which are now offered, however, will bear only upon two points in connection with the subject, and will naturally take some colour from the particular aspect of the question as applied to the city of Glasgow.

SYSTEM AND PRESSURE OF SUPPLY.

The fact that several undertakings which have commenced with an alternating single-phase high-tension supply have recently been actively engaged in changing their central or nearer areas to low-tension continuous current, supplied direct from dynamos fixed in the generating station without transformation of any kind, must surely be taken as a clear indication that low-tension continuous current is found to be more suitable and more economical for all purposes, where the distance is not so great as to make it too costly in copper. This change has, of course, been brought about by the alteration in the Board of Trade Regulations allowing the use of a pressure in consumers' premises not exceeding 250 volts, and consequently of a three-wire system with a pressure not exceeding 500 volts across the outers.

Mr. Addenbroke, in a very able paper read before this Institution in London a few years ago, foretold this result, and showed that with a favourably selected position of generating station an area measuring five miles in diameter could be economically dealt with by means of low-tension continuous current under these altered conditions. Experience has proved this to be so, and in Glasgow the supply is at present carried on over a radius of two and a half miles

each way from the New Station at Port Dundas. It does not follow, of course, that it will always be economical to work in this way, as the load may grow to such an extent as to warrant putting down other generating stations at the more distant points rather than transmitting many thousands of horse-power through cables from a generating station already fully taxed by a demand of some 20,000 to 30,000 horse-power within a radius of much less than two and a half miles around it.

On the other hand, there may be cases where small amounts of current require to be transmitted to exceptional distances, and in such cases small auxiliary high-tension plants, either continuous or alternating, would seem quite justifiable. The fact, however, that these may be necessary is no reason whatever why the whole generating plant should be made high-tension and current transformed down even in central areas close around the generating station.

The obvious common-sense principle on which to proceed is surely to use simple low-tension direct supply at as high a pressure as possible, and when this fails to meet the case, to supplement with high-tension transmission for the remainder.

A well-known consulting engineer, in a discussion on this subject some three or four years ago, went so far as to admit that this was the right line to go upon, but said that he found the public wanted an alternating-current supply and not a continuous one, and in consequence recommended that low-tension alternators be used for direct supply to the home areas and that step-up transformers be used in connection with this same supply for reaching the outlying districts, thus avoiding the necessity for any kind of separate generating plant for this purpose.

The idea was ingenious, but the author is not aware that any use has been made of it, possibly because it has been found that the public do not, after all, want alternating current.

For supplying motors or arc lighting who would prefer single-phase alternating current to continuous? And for incandescent lighting, what is to be gained by using alternating current?

It may be wasting your time to ask these

questions or to put these arguments before you, but no less a genius than Mr. Ferranti only recently publicly stated his opinion that single-phase alternating current would yet come to be the universally accepted system of supply.

There are, of course, those who say that two-phase or three-phase alternating current of low periodicity will shortly supersede everything else, but in their case the public are not found to want alternating current but continuous, and this is managed by means of rotary converters. The object of this system is said to be to allow the generating station (which for some reason, never yet satisfactorily explained, is bound to contain the whole generating plant of any one undertaking or more if possible) to be situated at a considerable distance from the area of supply, where land is cheap and coal readily obtainable.

In a place like London, where the cost of land within the lighting area is practically prohibitive, where railway accommodation is impossible and the conditions and restrictions arising through the interests of surrounding proprietors most onerous, there is no doubt some reason for adopting such a system; but it is somewhat remarkable that in the case of the Glasgow Tramway power supply, in which this system is being introduced, there was much anxiety to get a site, not as near the coal-pits as possible, but as near the centre of the city as might be. The site ultimately purchased is about the same distance radially from the centre of the city as the Port Dundas Electricity Supply Works, and figures were given to show both the extra capital which would be involved in going further afield and the annual extra cost through loss in distribution.

Again, it is argued that the weak spot in a continuous-current system is the commutator of the dynamo, but experience has proved that this is an absolutely erroneous idea. Nothing could be more satisfactory than the running of the brushes and commutators of most of the dynamos as now constructed, and, in fact, two- and three-phase alternating systems rely entirely upon commutators in the rotary converters.

The conclusions to which these considerations seem to

point are: (1) That there is no more economical or satisfactory method of supply than low-tension continuous current, generated upon a site or sites, within the area of supply if possible, and supplemented by a small amount of high-tension plant if necessary in order to supply remote corners of the area which may be beyond the reach of low-tension supply. (2) That if it be impossible to find sites within the area at any reasonable price and with railway accommodation and freedom from onerous restrictions due to surrounding proprietors, then a high-tension generating station or stations without the area will be the next best arrangement, though the cost of supply will be increased to some extent.

And it therefore follows that in order to ensure supply at the maximum economy in years to come, sites should be retained in suitable positions within, or as close as possible to the area of supply, and with proper railway accommodation and freedom from hampering restrictions, even at some sacrifice in the interval until they are required.

As regards the exact pressure of supply to consumers, it is obvious that the higher the pressure the more economical will be the distribution, and it must also be clear that where lighting and tramways are supplied from the same generating station it is an advantage to have the pressure for both purposes alike. The pressure allowed by the Board of Trade for tramway supply is 500 volts, and the dynamos are usually constructed to run from 500 to 550 volts for that purpose. These same dynamos will therefore be most suitable, if they are to be used alternatively on lighting mains, for supply on a 500-volt three-wire system, using 250-volt lamps in consumers' premises, with balancers or equalisers to compensate for out-of-balance middle-wire currents. They will allow up to 50 volts drop in feeders at times of heavy load. The Board of Trade Regulations for lighting place the limit for low-tension supply at 250 volts, but although the city of Glasgow has some 1,500 consumers with a maximum load of about 3,000 kilowatts already supplied at this pressure, and the municipalities of Govan and Greenock have also considerable numbers of consumers supplied at the same pressure with complete satisfaction, the Board of Trade are declining to sanction more than 240 volts declared press ndon district and elsewhere.

The argument appears to be that the 250-volt limit is one beyond which the pressure must on no account go, and that consequently it cannot be a declared pressure with the margin of variation allowed in another part of their Regulations.

Whatever may be the actual construction to be put upon the words from a legal point of view, surely it is unreasonable and inconsistent to take this line.

The Board of Trade is appointed to look after the interests of the public, but in what way do the public suffer through being supplied at 250 volts? The thing is done, and is an admitted success. It has been in operation in Glasgow for about eighteen months, and no trouble of any kind has arisen in connection with it. There can, therefore, be no scientific or practical reason why it should not be allowed.

The proposal to establish 240 volts means yet another voltage to worry manufacturers and contractors, and through them the public. If the Board of Trade had taken the line of establishing a standard voltage for the benefit of everybody concerned, there would have been some clear object in it, but the result of this decision will be to make at least five different standard voltages—viz., 200, 220, 230, 240, and 250 volts—in use in various places in the kingdom.

On the other hand, to agree upon 250 volts as the maximum declared pressure in consumers' premises would have tended to settle all new work down to this standard, and would have avoided the addition of another voltage to the already too numerous standards in use.

At a time like the present, when standardisation is so much talked about, electrical engineers will do well to give this matter special attention, and if necessary approach the Board of Trade in a body with a view to getting 250 volts sanctioned. It is hard, however, to see what necessity there can really be for taking so much trouble about so small a matter, as all that is required is for the Board of Trade to agree to that reading of Clause 1 in the Regulations for Securing the Safety of the Public, which shall allow 250 volts to be a declared pressure, subject to the variation of four per cent. from that pressure, as laid down in Clause 7 of the Regulations for Ensuring a Proper and Sufficient Supply of Electrical Energy.

Probably the real reason why electrical engineers have not taken any decisive action about this matter is that all the older undertakings are already committed to some other voltage, and will naturally not feel called upon to press for 250 volts when they themselves use 220 or 230, and would not find it worth while to exchange to 250. This, however, is short-sighted policy, and it seems to have resulted in the meantime in the standardisation not of any of the existing voltages, but of another one, viz., 240 for all new work.

In the case of Glasgow, the author of this paper was faced with the problem of converting an existing 200-volt three-wire system into one of a higher pressure, and the question immediately arose as to whether it would be right to be contented with a 400-volt system, using 200-volt lamps in consumers' premises, or to adopt any higher voltage.

While making an alteration, it was clear that if any departure were made from the simple multiple of the existing state of things, additional complications would arise in consumers' premises. The next consideration was whether the advantage to be gained was worth the complication and the cost thereof, and it then became necessary, in order to ascertain what the advantage would be, to settle upon some definite pressure as a basis for calculation. Granting that 250 volts could be used, it meant the difference between a 400- and a 500-volt supply, *i.e.*, an increase of 25 per cent. on the voltage. The value of this increase need not be enlarged upon in a paper addressed to electrical engineers. The advantages to be gained in the way of the saving of copper in the mains and ease of regulation of voltage over considerable distances seemed so great as to make it well worth while to take the step.

Glasgow always wishes to be in the front line, and it certainly would not have been so had it settled down contentedly to a 400-volt three-wire system at the point when it was starting to lay a distribution system, designed rapidly to extend over an area of some twenty-eight square miles. There is no doubt whatever in the author's mind that this alteration to 250 volts was the right step to take, and if the Board of Trade do not think so now, there cannot be the slightest doubt but that they will come to that opinion shortly. It is, however, much to be regretted that in the

meantime other new undertakings should be started in various places at 240 volts instead of 250.

There can be no cause for alarm or for the statement that it is a stretching of the Regulations which will never end, for it must be quite clear to any one that 250 volts is the absolute limit of declared pressure at customers' terminals (apart altogether from motive power supplies which are granted at 500 volts by the special permission of the Board of Trade), and no one could possibly go beyond this figure without deliberately defying the Regulations.

The author holds, of course, that the Regulations as they now stand are perfectly capable of being read so as to allow this 250 volts as a declared pressure, and he doubts very much whether any judge, after hearing arguments, would not decide that the real meaning of the Rules is that 250 volts may be used.

The words of the two clauses upon which the whole argument depends are as follows :—

> *A.—Pressure of Supply to Consumers.*
>
> (1) "The pressure of a supply delivered to any consumer shall not exceed 250 volts at any pair of terminals except with the express approval of the Board of Trade."
>
> *B.—Variation of Pressure at Consumers' Terminals.*
>
> "The variation of pressure at any consumer's terminals shall not under any conditions of the supply, which the consumer is entitled to receive, exceed 4 per cent. from the declared constant pressure."

For a time a great deal was made of the argument that 250-volt incandescent lamps could not be made satisfactorily. Before deciding upon the matter, therefore, the leading lamp manufacturers were consulted, and they all stated that there would be no difficulty, and that they would be very pleased to take orders for such lamps. For the most part they have been as good as their word, and although some makers have not been quite successful as yet, they will no doubt shortly be able to fall into line with their competitors. Lamps of even five and six candle-power have been in use in Glasgow for over fifteen months past at 250 volts without failure.

The efficiencies of the good lamps now in use are:—

5 and 6 c.p.	5	watts per candle.
8 c.p.	4	" "
16 "	4	" "
32 "	$3\frac{1}{2}$	" "
50 "	$3\frac{1}{4}$	" "

There seems to be but little in the old idea that thin filaments could not be made to last. The leading manufacturers seem to have so far conquered their original difficulties in this respect that even 250 volts does not seem to be the limit to which they can go. Indeed it seems doubtful whether in attempting higher pressures the proximity of the terminals in the present design of lamp would not cause greater trouble than the thinness of the filaments. Up to 250 volts, however, no trouble has occurred in this respect.

There is another possibility to be borne in mind in connection with this matter, and that is the introduction in the near future of the Nernst incandescent lamp. The construction of the filament in this lamp lends itself most readily to the use of 250 or even 500 volts.

The possibilities which lie before us with the Nernst incandescent lamp, giving the necessary light with about half the amount of energy consumed in the present form of lamp, combined with the advantages of the 250-volt supply, are great.

A word may not be out of place on the vexed question of interfering with existing arrangements in some consumers' premises, such as two ordinary open-type arc lamps in series on 100 volts, special small motors for dentists' use, low-voltage heaters and cauteries and such like complications.

One frequently meets the argument that the increase in pressure is all for the benefit of the Corporation, and not for the benefit of the consumer. This argument is of course used by those who think that they will suffer by the change; but they are a very small minority, and do not realise that the object of the change is the cheapening of the supply all round.

It is absurd to talk about "the benefit of the Corporation." Who are the Corporation but the representatives of the people, and whose is the undertaking but the people's? The object of " [illegible] -ation is to give that form of supply

which is most economical and consequently most convenient, for the majority. Electric light has been in the past too much the exclusive light of the rich man, who can well afford to pay a high price and to indulge in every kind of low-voltage complication which suits his own special fancy or convenience; but to maintain low voltage to suit these consumers means to keep up the price and to prevent the great British public from using electric light at all. The benefit of the majority, while at the same time dealing justly with existing consumers in making the change, is what the Corporation aims at. There are no dividends to make, and all profits are used, after providing for the lasting financial soundness of the undertaking, in reducing the price.

The Corporation might have been content to leave things as they were and let the price remain high, but this would have prevented the growth and spread of the undertaking, leaving the supply to those who could afford to pay for it, and whose special and exceptional circumstances it happened to suit.

Most people want light, and they want it in 8, 16, or 32 candle-power lamps. These, therefore, demand the first and most careful consideration. The question is how to supply these at the lowest cost. Having settled that, the other consumers must be made to accommodate themselves to the altered condition of things, and it is surprising, after all, how very little difficulty is experienced in modifying existing appliances to suit the new conditions.

Voltmeters are met with in several houses supplied at 100 volts. They are not readily altered to suit 250 volts, but why should they be allowed to interfere with progress? They are at best most unnecessary and objectionable things for consumers to have. It is a significant fact that it is only in 100-volt supplies as a rule that these voltmeters are found.

In concluding, attention should be drawn to the anomalous position in which the Board of Trade have put the owners of undertakings by allowing them to increase the pressure of supply to all consumers, but only with the consent of such consumers in the case of those supplied previously to the date of the revised Regulations.

In Glasgow common sense has always so far prevailed, but in London it does not seem to have done so, and a mere handful of consumers have in some cases held out against

the change for no apparent reason whatever, except the unpleasant peculiarities of certain kinds of human nature.

A deputation representing some of the London Supply Companies waited upon the President of the Board of Trade about the matter. While admitting that it seemed unfair for a few consumers to have the power of practically upsetting the whole undertaking, the President is reported to have said that the companies should have done more to conciliate the consumers. One cannot help feeling that the companies in London are hardly dealt with. Every improvement they try to make seems to be obstructed by County Council, Board of Trade, or some other body. They are supposed to be doing everything purely for their own selfish gain, and never with the object of improving the conditions for the consumer. Would not the case be very different if the local authorities of London all held the electrical undertakings in their own hands? Would the President of the Board of Trade have made the same remarks if the local authorities had been making the representation before him? Yet the necessity for the change is the same with the companies as with the undertakings in the hands of local authorities.

It is to be hoped that the Board of Trade will give compulsory powers in this matter of change of voltage, subject to arrangement or compensation to be settled by an independent arbitrator.

The local authorities of London may quite possibly be the right parties to own the electricity undertakings, and it is their own fault that they are not so.

Glasgow has realised the importance of the matter, and purchased the original undertaking from Messrs. Muir, Mavor, and Coulson, and more recently the Kelvinside Company's undertaking. The whole of this latter company's consumers have now, with one exception, been changed to the 250-volt supply, and great progress has been made with the change of the old central area. Govan, which is not at present part of Glasgow, has started its own supply at 250 volts, so that if ever it is annexed it will join up with Glasgow without further change. In this way the whole of Glasgow and the adjoining communities are in a fair way to get a universal supply at 250 volts, with the exception of Partick, which is said, under the decision of the Board of

Trade, to be laying down a supply at 240 volts. This is not good, but Glasgow may nevertheless congratulate itself that it is not like London.

BAILIE MACLAY, convener of the Glasgow Corporation Electricity Committee, said that he was very pleased to be present to hear Mr. Chamen's able paper. They of the Electricity Committee had engaged Mr. Chamen under a deep sense of responsibility, but he thought that anybody who had seen the work that Mr. Chamen had done for them, would agree that they could not possibly have made a better choice. He was very pleased to have heard Mr. Chamen's views expressed to a scientific audience, and from the attention with which they had been followed it was clear to him that he could express himself equally lucidly to a technical audience or to the members of the Electricity Committee. Bailie Maclay.

Mr. W. B. SAYERS said that he agreed with Mr. Chamen that continuous current is the most satisfactory supply for a consumer. Beyond the reasons for this given by Mr. Chamen there was the additional reason that continuous-current motors may be made with variable speeds economically. Until recently difficulties of commutation made it impracticable to work a continuous-current motor at anything below its normal excitation without loss of power and destruction of the commutator, but recent improvements had rendered it possible to vary the speed of a motor. This the speaker thought was a very great advantage. Motors which were installed for continuous current might be made to drive a machine not at *nearly* the correct speed but at *exactly* the correct speed. With reference to the question of voltage of supply to consumers and the Board of Trade limitation to 240 volts, it seemed only reasonable that the Board of Trade should allow in the limit of pressure to private consumers the same latitude as they recognise with regard to the "declared" pressure. In other words they would be perfectly consistent if they specified that the limit should not be deemed to be exceeded if the pressure were within four per cent. of 250 volts. This seemed to him (Mr. Sayers) to be the natural and common-sense decision for the Board of Trade to come to on the question. Mr. W. B. Sayers.

With regard to Mr. Chamen's reference to the firm of Messrs. Mavor & Coulson, he (Mr. Sayers) did not think that they had been sufficiently remembered for their enterprise and the pecuniary loss they had sustained when their station, mains, etc., were taken over at scrap value. Mr. Mavor had told him of occasions when, seated in his office, watching the arc lamps in a building opposite, his heart would jump with any flicker of the light, lest a hitch had taken place in the supply. Messrs. Muir, Mavor & Coulson's venture was the beginning of the Glasgow public supply, however, and he thought their pioneer work in this connection deserved to be remembered by the citizens of Glasgow.

Mr. M. T. PICKSTONE, of Messrs. D. Bruce Peebles & Co., Edinburgh, referred to the variation in voltage required for continuous-current generators. He said this could economically be obtained without sacrificing Mr. Pickstone.

Mr. Pickstone.

efficiency to a very large extent. His firm had frequently had to produce generators running with a range of from 400 to 500 volts with constant engine-speed, and in fact it seemed to him that the only point that affected central stations was the question of the lamps, and so far as the station was concerned there was every possible advantage in using a higher pressure.

With reference to the subject of speed variation, he had a conversation with Mr. Chamen about a year ago as to the possibility and extent to which the field of a generator could be weakened and still allow its maximum current to be extracted. Mr. Chamen expressed the opinion that he thought it possible with certain types of machines to weaken the field down to zero and still take the full current without difficulties as to sparking at the commutator. This of course is done to a very great extent in all central stations in connection with the battery-charging boosters; he had, however, been experimenting of late in this direction with the view of utilising it for variable-speed motors, and at that moment his firm had in hand a contract for supplying Messrs. Lloyds, of London, with a printing press motor of 50 H.P. worked on this plan, which is commonly known as the Ward-Leonard system, in conjunction with Messrs. Geipel & Lange.

With this system of motor control it is possible to start the large motor from rest and get a gradual variation in speed up to the full load from zero, the amount of variation between each step merely depending upon the number of contacts on the shunt rheostat used. In the case in point a small motor-generator capable of carrying the full current of a 50-H.P. motor is provided, the generator portion of the motor-generator being provided with a shunt-reversing rheostat. The switchgear for this is exceedingly simple, and merely consists of a starting switch for the motor-generator, a reversing shunt rheostat for the generator portion of the transformer, and a double pole circuit breaker in connection with the large machine. The transformer is first switched on, the generating portion developing 200 volts opposing the voltage of the line, and thus preventing any current from passing through the large motor. On gradually weakening the generator field, the large motor slowly starts from rest with its full current, and the speed is raised by weakening the generator field until this latter is at zero. At this instant the large motor is running at half its full speed, and the generator is carrying its full-load current with no field whatever. Still further to increase the speed of the motor, the shunt of the generator is reversed and the field strengthened up in the opposite direction until the full field in the opposite direction is obtained on the generator. During this latter series of operations the voltage of the generator is strengthening the voltage of the line, so that the large machine has practically 400 volts across it when running at full load. With this system of motor control not only is the switchgear exceedingly simple and compact, but there is no possibility of the motor-man making any mistake regarding it. The efficiency of the whole system is, moreover, exceedingly high, and the writer hopes in the course of a few months to be able to give the Institution a careful series of tests as regards efficiency and regulation of such combinations. This system of course is not new, having

been adopted to some considerable extent in the United States, but it does not appear, for some reason or other, to have been made use of in this country before, and more light is needed for the public to appreciate its manifold advantages. Mr. Pickstone.

Mr. J. M. M. MUNRO: The paper does not offer to deal with the whole subject of electrical distribution, or even exhaustively to review any definite detail of that subject. Mr. Chamen has chosen instead to deal with a few questions, primarily of local interest, but not without very great general importance. He has discussed these with that practical, comprehensive common sense which we have learned to expect from him. The relative advantages of high and low tension for town lighting is a subject which lends itself to endless discussion, as each disputant can select, out of endless possibilities, the conditions favourable to the system he favours, and so have no difficulty in proving that system to be the more economical. But when work is begun in a definite area, facts assert themselves. Mr. Chamen is right, therefore, in considering it a suggestive fact that several towns are changing, or are desirous of changing, from high-tension alternating to low-tension continuous. A considerable part of Mr. Chamen's paper is devoted to defending the pressure of 250 volts adopted in Glasgow, and deprecating the decision of the Board of Trade elsewhere to enforce 240 as a maximum. I am not going to enter on the legal question of the interpretation of the Board of Trade rules. But it is quite within our province to discuss whether there be any practical reason why 250 volts should or should not be a permissible declared pressure. Reasonable objection might be made to this pressure if it could be shown either—(1) That the shorter life and slightly lower efficiency and other practical disadvantages of the 250-volt lamp more than neutralised the economy of generation and distribution due to the difference between 150 volts and any lower pressure. (2) That the risk of leakage, fire, extinction of light, &c., was so increased that the whole system, outdoor and in, could not be made as safe as before without adding so much to the capital expenditure on insulation that the interest and depreciation thereon would neutralise the saving of copper by use of the higher pressure. (The Glasgow installation, as Mr. Chamen has pointed out, belongs to the citizens, so that the sum of the losses of (1) and (2) should be set against the initial saving due to the increased voltage.) (3) That the risk of injury by shock was seriously increased by the increase of potential difference. Mr. Munro

So far as I have been able to make out, after endeavouring to balance these with such figures as I was in a position to obtain, the benefit of using 250 volts amply justified the change from 100 or even 200 volts. It is obvious that a pressure of 240 volts gives nearly all the benefits which may be derived from 250. Why, then, apart from the question of too many standards (for the variety of which the Board of Trade is not altogether responsible), ought we to agitate for a 250-volts standard? My own mind has for many years favoured the adoption of 250 volts as the maximum standard; for these among other reasons—(1) Its double (500–550 volts), the pressure of the outers, is the common standard pressure for tramway work. In most places the supply for

Mr. Munro. tramways and lighting can be with advantage combined under one management, and it is as well to have as many parts as possible interchangeable between the two. (2) The pressure of 500–550 volts is about the limit of safe pressure for physiological reasons. At this pressure shock is unpleasant. We have all heard of injury to people by much less than 500 volts, but we also know of many who have received 500-volt shocks without harm. Much of course depends on the route of the current through the body, the resistance of the contact made, and the condition of the recipient both as regards mind and body. In any case a pressure of 500 volts appears to be about the safe limit under ordinary conditions. (3) A less important reason is that 250–500 are conveniently round figures. (4) I might add a fourth reason that with pressures over 250 volts switches and minor appliances need to be somewhat clumsy to get safety distance between parts.

I have no doubt that it will in time be necessary to add to the number and proximity of Glasgow supply stations. I am sorry the new Electric Tramway Station was not farther from the Electric Light Station at Port Dundas—much farther, or a great deal nearer—that is, under one roof.

For long-distance transmission, the various forms of alternating high-tension current may be, and are, successfully employed. Something has been attempted in this country, and more will be done, in the direction of sending power from where it is cheap to where it is dear. For electric railway work alone, on other than short, isolated lines, there is a great future for transmission at high pressure, with transformation down to working pressures of 500 volts for polyphase motors, as well as by transformation to continuous current at like pressures for lines having many stopping-places close together.

Lord Kelvin. LORD KELVIN asked Mr. Chamen if it was possible to increase the pressure of an installation from, say, 200 volts to 250 volts by simply increasing the speed of the engines and dynamos. He wished to know if the fittings now in use for 100 or 200 volts were suitable for 250 volts. Lord Kelvin had himself said some years ago that he did not think it would be safe to allow a higher pressure than 300 volts inside a consumer's premises. His Lordship also made interesting reference to the development of the incandescent lamp by Swan and Edison. He mentioned that Swan had started by making lamps at 45 volts pressure, and that Edison had doubled this pressure, making his lamps suitable for 100 volts. Since then the pressure of supply had gradually risen, and he was pleased to know that satisfactory 250-volt lamps could now be made.

Mr. Ionides. MR. P. D. IONIDES, of the Westinghouse Company, made brief reference to the flexibility of a three-phase transmission and distribution.

Mr. Mavor. MR. SAM MAVOR, referring to Mr. Sayers' remarks, wished to acknowledge the very generous recognition which Mr. Chamen had always accorded to Messrs. Muir, Mavor & Coulson's pioneer work in the Public Electric Supply for Glasgow. Mr. Chamen had earned great credit [illegible] courage in taking the initiative in boldly adopting the maximum [illegible] allowed by the Board of Trade, and in facing

not only such technical difficulties as existed but also the uncertainties involved in his interpretation of an ambiguous clause in the Board of Trade Regulations. That Mr. Chamen's rendering of the clause was the common-sense one the speaker had no doubt, and he was glad to hear that Lord Kelvin gave the weight of his authority in support of this interpretation. Mr. Mavor.

Bailie Maclay had referred to the Corporation's good fortune in having the professional advice of Lord Kelvin at the critical time when the electric lighting undertaking was absorbed, and especially to the advice which led to the adoption of the low-tension system. Although it was now ancient history, the speaker reminded Lord Kelvin that Messrs. Muir, Mavor & Coulson also had the privilege of his advice and were in a position, with his Lordship's support, to inform the Corporation that whatever might be the best course for *them* to follow, the high-tension alternating system of distribution was the only commercially possible one by which the pioneer work could have been carried out, and it was therefore the right one for Muir, Mavor & Coulson to adopt. In valuing the high-tension plant, however, the Corporation omitted to value the business it had made; but the value of the business they then took over has since dawned upon them.

Professor M. MACLEAN gave two tables showing the pressure of supply in 90 low-tension stations and in 75 high-pressure stations in this country. Of 90 low-pressure continuous-current stations he found that there were— Professor Maclean.

10	at	100	volts	declared pressure.	
1	„	107	„	„	(St. James' Parish).
1	„	113	„	„	(Leamington).
4	„	110	„	„	
4	„	150	„	„	
13	„	200	„	„	
15	„	210	„	„	
24	„	220	„	„	
14	„	230	„	„	
1	„	240	„	„	
3	„	250	„	„	namely—(*a*) Glasgow, (*b*) Greenock, and (*c*) Govan.

but of 75 high-pressure alternating current stations there were—

28	working at	100	volts pressure.	
2	„	102	„	
1	„	105	„	
35	„	200	„	
2	„	205	„	
2	„	210	„	
3	„	220	„	
1	„	230	„	
1	„	250	„	(Bristol).

Mr. JAMES COATS (*communicated*): With reference to the number of M

Mr. Coats. electric supply companies using high-voltage continuous current, Dr. Magnus Maclean mentioned there were only three of 250 volts, viz., Glasgow, Govan and Greenock. I may mention, however, that the Scottish Co-operative Wholesale Society, Limited, of Glasgow (while not a public supply company, yet having a larger output than many public companies), were among the first in Scotland to adopt the higher pressures. It is now about five years since I observed the St. Pancras Vestry had raised their voltage to 220—Glasgow at that time being 100 volts. About that time I laid down a fair-sized lighting installation at a pressure of 220 volts, which has now been working for four years without having to renew a switch. Two years ago a larger installation was required for lighting and transmission of power in connection with six large factories, having over 30 motors ranging from $\frac{1}{8}$ H.P. to to 20 H.P. I decided to follow on the lines of Mr. Chamen, and the plant accordingly was laid down for 250 volts. This has now been running for sixteen months amongst all classes of workpeople, male and female, and so far everything has been very satisfactory. Apart from the considerable saving in the cables my main object in going up to this voltage was to be in keeping with the Glasgow Corporation supply, so that there should be no difficulty in changing over at any time that the Corporation were able to supply the current cheaper than we could produce it. It seems to me it is most absurd for the Board of Trade to issue vague regulations, which apparently can only be read by lawyers.

Mr. McWhirter. Mr. William McWhirter (*communicated*): I must first of all congratulate the Glasgow Local Section on having a paper brought before them dealing so admirably with electricity supply so far as Glasgow is concerned. With the arguments put forward by Mr. Chamen in favour of low-tension continuous current, as against alternating high tension, in compact areas I think every one must agree, it is surprising, in face of all the experience gained with low-tension working, that even now engineers are to be found who have the courage to advise the adoption of alternating currents in such areas.

No fault can be found with Mr. Chamen's decision to adopt the full pressure allowed by the Board of Trade, and in my opinion it is a grievous mistake that the Board of Trade should now step in to make an alteration of only a few volts, which, however, are sufficient to interfere with what was most likely in the future to be a universal standard of pressure, viz., 500 volts on a three-wire system. Surely the time is come when the electrical trades should take up a position and bring pressure to bear upon the Board of Trade in such a way that so unreasonable a proposal as this alteration should not take effect. Other trades and industries have had before now to bring such pressure to bear upon a Government department, and surely now is the time for the electrical industry to be up and doing. Supposing the Board of Trade argument to be that 500 volts *plus* the drop in the feeders is dangerous, then surely one and all will admit that 500 volts cannot be safe. That being so, it is much the same as allowing an alternating-current supply at 1,000 volts, but refusing to pass it at 10,000 volts. Mr. Ch[illegible] very clever argument in favour of

the increased pressure when he defines "who are the Corporation." There is no doubt whatever that with all deference to such an authority the pressure of 250 volts is not so convenient for the user as a rule, but at the same time there is every prospect that this will be only a temporary objection, as no doubt very shortly lamps will be found (especially arc lamps) which will work as well, and as economically, on the higher pressure as they have hitherto on the lower. Failing this there is no reason why the reduced price of electricity when used in motors should not be utilised in many instances for driving small motor generators, and so getting the pressure that may be found most convenient. Mr. McWhirter.

In conclusion I have to join in thanking Mr. Chamen most heartily for the splendid paper he has brought before the meeting.

Mr. W. A. Chamen, in reply to the discussion, said that the only difficulty in making a change of pressure lay in the consumers' installations. Even if incandescent lamps were a little less efficient at the higher voltage, that had been taken into account by Mr. Addenbroke in the paper referred to, and there was still a considerable balance in favour of the high pressure as regarded cost of lighting, because the price of current could be so much reduced. Mr. Chamen.

Experience had not shown that there was any greater risk of fire with 250 volts than with 100 volts. What had certainly given him some trouble with bad installations (often new ones) was the fact that the middle wire of the 250-volt system was earthed.

This caused small outbreaks of fire sometimes by fusing compo gas-pipes and setting the gas on fire.

The risk of accident by shock was nothing, but it had often struck him as strange that while the Board of Trade were so anxious lest the public should get shocks they seemed perfectly indifferent about the risk of burning people alive in their own houses. That question appeared to be the business of some one else.

As regarded the question why 250 volts should be maintained as against 240, which the Board of Trade were prepared to sanction, it must be remembered that at the time 250 volts was made use of in Glasgow there was nothing above 230 in use elsewhere. Two hundred and forty volts had not been suggested by any one, and the Board of Trade had deliberately made that fresh standard after Glasgow had already started and succeeded with a 250-volt supply.

In answer to Lord Kelvin's questions it was generally found that the lamp-holders and switches were quite good for use with the increased pressure on installations which had been put in during the last four or five years.

There were, however, some lamp-holders of ancient date still found in use occasionally, and also some open switches without covers, which had to be replaced. Generally speaking any good make of switch with a quick break would work quite well under the increased pressure so long as there was no increase made in the number of lamps controlled by it.

Dynamos constructed to run at 220 volts could quite well be run up to 250 volts as a rule, but the difficulty arose not so much with the

Mr. Chamen dynamos as with the engines which might not so well stand the extra speed.

He was sorry that Mr. Ionides had been alone in the defence of multiphase current supplies, and thought he ought to have been better supported. No doubt multiphase current could be utilised for driving variable speed motors to some extent, and also for electric furnaces, but there did not appear to be any particular advantage in the use of it, even for these purposes, and it must be remembered that the main question now under consideration was the general supply of electricity for lighting firstly, and then for any other purposes which might be made to fit in.

With regard to the question of costs it must be remembered that no fine economy had been possible in Glasgow for some time past, where the supply had to be carried on under conditions of great difficulty in works under construction.

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The Three Hundred and Fifty-Second Ordinary General Meeting of the Institution was held at the Institution of Mechanical Engineers, Storey's Gate, Westminster, on Thursday evening, November 29th, 1900—Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 22nd, 1900, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that these names should be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Associate Members—

Thomas Ernest Herbert.

From the class of Students to that of Associates—

John Frank Auguste Margetts.

Messrs. J. H. Johnson and S. J. Clay were appointed scrutineers of the ballot for the election of new members.

A donation to the Library was announced as having been received since the last meeting from Société Anonyme John Cockerill, and to the *Building Fund* from Mr. James Kynoch, to whom the thanks of the meeting were duly accorded.

The PRESIDENT : I have to announce that on the 18th of December a Reception will be held at the Covent Garden Opera House, which has been most kindly lent for the

purpose by the Managers, Mr. Rendle and Mr. Forsyth. The Institution will then, assisted by the Corps of Electrical Engineers, Royal Engineers, Volunteers, receive the Active Service Contingent of that Corps on its return from South Africa. The 18th of December has been chosen because, although the return of the South African Detachment is, I believe, announced for the 8th or 9th of that month, there are delays in ships, and we felt it was well to have a safe date. Notices will be sent to members in the ordinary course.

I will now ask Mr. Langdon, Vice-President, to read his paper.

ON THE SUPERSESSION OF THE STEAM BY THE ELECTRIC LOCOMOTIVE.

By W. LANGDON, Vice-President.

Probably one of the most interesting questions associated with the application of electrical energy to railway work is its eventual supersession of the steam locomotive. Electric locomotives of a capacity equal to that of the steam locomotive, doing similar work, and possessing certain marked advantages, are an accomplished fact, and to many it may seem that the days of the steam locomotive are numbered. We must not, however, forget that that which has been done has its *raison d'être*. Its employment has hitherto been generally confined to localities where the effects of steam and smoke would exercise a baneful influence. Because it has been so employed, and its employment has been attended with such marked success, it does not follow, however much it may appear desirable, that in overland lines of railway electricity will in future prove to be the element of power. Railways are commercial undertakings, in which vast sums of money have been embarked, all of which, investors expect, will produce a certain annual return. The supersession of the steam by the electric locomotive thus resolves itself, primarily, into one of profit and loss. If its adoption will enable railways to be worked more economically than is the case under steam, then it will, with its attendant advantages, sooner or later, be adopted—if not by the whole, certainly by the greater portion of the railways of

this country; but, whatever the ulterior advantages may be, unattended by this result, its adoption will remain doubtful.

The first question to be asked is: Are we in a position to consider the subject? Is the data at our disposal such as to admit of a reasonable treatment of it? The primary factors are well known. We know how many pounds of steam we can obtain from a given quantity of coal. The efficiencies of prime movers and electrical generators, the loss in transmission, transformation, and distribution are all determinable. Any advantages yet to come must be looked for in the steam generator and in the simplification and consequent ultimate increase of efficiency in the electrical apparatus. In each, no doubt, the future will see production cheapened. Labour may advance, but competition will grow, and in the price of the manufactured article we may reasonably look to the future to produce some advantage.

Having then the material at hand, it is reasonable to conclude that the consideration of the subject can only be attended with good; for, if it should be shown that advantage is to be anticipated from such a change, it will help us to grasp that which has to be attempted, and perhaps to evolve from the data at our disposal the course most desirable to follow.

The railways of the United Kingdom comprised on the 31st of December, 1899,[1] the following mileage of line:—

Double Line or more	11,977
Single Line	9,723
Total mileage	21,700

The number of locomotives employed for working these railways was 20,461. The total number of vehicles of all descriptions was rendered as 752,930. The year's cost for locomotive power, including stationary engines, was £16,491,377. The number of miles travelled by trains was 396,241,265. Number of passengers—exclusive of season tickets—1,106,691,991. The tonnage of minerals, 296,611,190; and of general merchandise, 117,011,835.

In dealing with a question of this nature it seems

[1] Board of Trade Railway Returns, 1899.

desirable to produce these figures; but they are, in fact, except to afford some idea of the magnitude of the subject, of very little use. The length of the passenger journeys is not stated, nor are the journeys taken by season ticket holders included. To be of value the per passenger mileage, including of course the journeys made by season ticket holders, should be given. The same objection applies to the mineral and goods tonnage statement. It records the tonnage declared as placed upon the railway, but whether it is carried one mile or one hundred miles is not shown. In fact the returns, as rendered, aid the consideration of the subject very little; and I am sorry to say the information in the hands of the railway companies themselves carries us no further.

Under these circumstances, the establishment of a common basis upon which to consider the relative cost presents much difficulty. The only manner in which it can be approached is by averaging the data obtainable, or by examining the work of a particular section of line.

In Table I., I furnish extracts from the Board of Trade Returns for the year 1899, showing the mileage of line, cost of locomotive power, train mileage worked, &c., for England and Wales, Scotland and Ireland, together with certain deductions therefrom applicable to these results; as also similar data in reference to six of the most important English companies.

In considering the mileage of line quoted, it should be metioned that the figures do not disclose the *mileage of roads.* Over many sections of the several lines of railway there are more than two roads. Most main routes working out of London possess four, and even six roads, for a considerable distance; and the same applies to other busy portions of the chief railway systems.

The table indicated shows that the cost of locomotive power for the United Kingdom, including stationary engines for pumping and other purposes, works out at 9·988 pence per train mile. That the cost for England and Wales is 10·253; for Scotland 8·862; and for Ireland 8·104 pence. And that this cost varies with the several companies quoted as follows:—London and North-Western, 9·477 pence; London and South-Western, 9·844; Great Northern, 9·892; Great Western, 9·905; Midland, 10·218; and North-Eastern, 12·266—their average cost being 10·267.

Table I.

MILEAGE OF LINE, COST OF LOCOMOTIVE POWER, TRAIN MILEAGE, ETC.,

For Year Ending December 31, 1899.

(a)	Length of Line in Miles open on 31st December, 1899. Double or more.	Single.	Total. (b)	Cost of Locomotive Power, including Stationary Engines. (c)	No. of Locomotives. (d)	Number of Miles Travelled by Trains. By Passenger Trains. (e)	By Goods and Minerals. (f)	Total. (g)	(h) Cost per Train-mile for Locomotive Power. $\frac{c}{g}$	(k) Number of Miles Travelled by Trains per hour during year of 365 days. $\frac{g}{8,760}$	(l) Number of Trains per Mile per hour (365 days to the year.) $\frac{k}{b}$
				£					Pence.		
England and Wales	9,933	5,111	15,044	14,101,842	17,411	178,684,803	151,377,966	330,062,769	10·253	37,678	2·505
Scotland	1,423	2,057	3,480	1,811,552	2,241	27,588,633	21,473,089	49,061,722	8·862	5,601	1·609
Ireland	621	2,555	3,176	577,983	809	10,367,617	6,749,157	17,116,774	8·104	1,954	0·615
Total—United Kingdom...	11,977	9,723	21,700	16,401,377	20,461	216,641,053	179,600,212	396,241,265	9·988 Average.	45,233	2·084 Average.
				Railway Companies.							
				£					Pence.		
Great Northern			825	995,690	1,259	11,729,522	12,426,497	24,156,019	9·892	2,757	3·3
Great Western			2,602	1,884,032	1,933	23,085,687	22,561,470	45,647,157	9·905	5,210	2·0
London and North-Western			1,924	1,927,049	2,959	26,285,278	22,515,697	48,800,975	9·477	5,570	2·9
London and South-Western			900	709,110	728	12,598,390	4,689,644	17,288,034	9·844	1,973	2·2
Midland			1,431	2,016,816	2,507	18,979,594	28,387,090	47,366,684	10·218	5,407	3·7
North-Eastern			1,632	1,632,967	2,047	14,641,572	17,307,802	31,949,374	12·266	3,647	2·2
									Average 10·267		

Column *l* indicates the number of trains, per mile per hour, based upon the mileage of line of railway shown by the returns. Bearing in mind that there are, over many portions of the railways indicated, several lines, or roads, the number of trains travelling per mile per hour is, perhaps, surprisingly small. Ireland claims but 0·615; Scotland, 1·609; England and Wales, 2·505. Of the railways individualised, the Great Western has an average of but 2; the London and South-Western and the North-Eastern each 2·2; London and North-Western, 2·9; Great Northern, 3·3; and the Midland, 3·7 (that is, the Midland, for instance, has, on an average, 3·7 trains passing over each *mile of line of railway*, each hour; not that the mile of line is occupied by these 3·7 trains during the entire hour, but that that is the average number of trains worked over the mile of line of railway—quite irrespective of the number of lines of rails—within that period of time).

If trains were, in practice, so distributed, this might form a basis on which to found a comparison of cost for the entire railway system; but we know that this is not so—that trains are much more frequent on certain portions and less frequent on other portions of the lines of railway, and although it is a kind of basis, affording some very interesting figures, it is not, as it stands, a practical one for the purpose in view. To effect this it is necessary to consider the work of an individual section of some line of railway; and with this view I have taken that portion of the Midland Company's main line between London (St. Pancras) and Bedford—omitting the suburban and local traffic applicable to the Metropolitan and Tottenham lines.

The length of line of railway is 49·5 miles—practically 50 miles; and the number of lines of rails applicable to the traffic under consideration may be taken as four—it is not less. In order to ascertain the number and character of trains to be taken into consideration, I have obtained returns (Tables II. and III.) extracted from the Block Book, of those trains passing two points—Luton and Harpenden—each hour, during a day of twenty-four hours in the month of July, 1900, which I have summarised and classified so as to bring the subject within reasonable scope. These details are carried forward to Table IV., which forms the basis of

calculation for comparison of cost for power to work the traffic indicated.

TABLE II.

LUTON.

Statement of the Number of Trains recorded in the Block Book for the 24 hours ending midnight on Thursday, July 19, 1900.

Hours.	Express Passenger.	Ordinary Passenger.	Coaches.	Fish and Milk.	Express Goods. Class "A."	Express Goods. Class "B."	Ordinary Goods.	Minerals.	Light Engines.	Total number of Trains each hour.
12 to 1 a.m. ...	2	—	—	—	1	5	—	3	—	11
1 " 2 " ...	—	—	1	—	—	10	—	2	—	13
2 " 3 " ...	—	—	1	3	1	3	—	5	—	13
3 " 4 " ...	1	—	—	—	—	3	—	6	—	10
4 " 5 " ...	1	—	—	—	3	5	—	6	—	15
5 " 6 " ...	2	—	—	—	2	4	1	2	—	11
6 " 7 " ...	1	1	—	—	3	—	1	5	—	11
7 " 8 " ...	1	1	4	—	2	4	1	3	—	16
8 " 9 " ...	1	1	—	—	1	2	—	2	—	7
9 " 10 " ...	4	—	—	—	—	2	—	4	—	10
10 " 11 " ...	4	1	—	—	—	1	1	4	—	11
11 " 12 (noon) ...	3	—	—	—	1	1	1	3	—	9
12 " 1 p.m. ...	4	1	—	—	—	1	2	5	—	13
1 " 2 " ...	4	1	1	—	—	2	—	4	—	12
2 " 3 " ...	6	2	3	—	—	—	—	8	—	19
3 " 4 " ...	2	3	—	—	1	—	3	5	—	14
4 " 5 " ...	4	—	1	—	—	—	1	5	—	11
5 " 6 " ...	4	2	—	—	—	—	2	3	—	11
6 " 7 " ...	5	1	—	—	—	—	—	4	—	10
7 " 8 " ...	4	2	1	—	—	—	—	5	—	12
8 " 9 " ...	—	2	1	—	3	—	—	1	—	7
9 " 10 " ...	2	—	—	—	1	2	—	4	—	9
10 " 11 " ...	5	2	—	—	—	—	2	7	—	16
11 " 12 (midnight)	2	1	2	—	1	6	3	1	—	16

CLASSIFICATION.

Express Passenger...			62
Ordinary Passenger		21	36
Coaches		15	
Fish and Milk		3	74
Express Goods—Class "A."	20	71	
" Class "B."	51		
Ordinary Goods		18	115
Minerals		97	
Total number of Trains during the 24 hours ...			287
Maximum number of Trains in 1 hour			19
Minimum " " "			7

TABLE III.

HARPENDEN.

Statement of the Number of Trains recorded in the Block Book for the 24 hours ending midnight on Thursday, July 19, 1900.

Hours.	Express Passenger.	Ordinary Passenger.	Coaches.	Fish and Milk.	Express Goods. Class "A."	Express Goods. Class "B."	Ordinary Goods.	Minerals.	Light Engines.	Total number of Trains each hour.
12 to 1 a.m. ...	2	—	—	—	3	3	1	4	—	13
1 " 2 " ...	—	—	1	—	—	9	2	2	—	14
2 " 3 " ...	—	—	1	3	1	3	2	1	—	11
3 " 4 " ...	—	—	—	—	2	3	3	3	—	11
4 " 5 " ...	1	—	—	—	2	5	4	1	—	13
5 " 6 " ...	3	1	—	—	2	4	2	1	1	14
6 " 7 " ...	—	—	—	—	—	2	5	1	1	9
7 " 8 " ...	2	3	2	—	2	5	—	—	1	15
8 " 9 " ...	4	1	—	—	1	—	5	3	—	14
9 " 10 " ...	5	2	—	—	—	1	3	1	—	12
10 " 11 " ...	3	1	—	—	1	1	—	1	—	7
11 " 12 (noon) ...	5	—	—	—	—	2	2	2	—	11
12 " 1 p.m. ...	5	2	1	—	—	1	1	2	—	12
1 " 2 " ...	3	2	—	—	—	1	6	3	1	16
2 " 3 " ...	5	3	1	—	—	1	3	2	1	16
3 " 4 " ...	4	1	1	—	—	1	2	3	1	13
4 " 5 " ...	3	5	—	—	—	—	2	2	—	12
5 " 6 " ...	5	1	—	—	—	—	2	2	—	10
6 " 7 " ...	3	1	—	—	—	—	2	2	1	9
7 " 8 " ...	6	2	—	—	2	—	1	—	—	11
8 " 9 " ...	2	3	—	—	1	2	1	3	1	13
9 " 10 " ...	3	2	—	—	—	3	3	3	1	15
10 " 11 " ...	3	2	—	1	1	1	1	2	1	12
11 " 12 (midnight)	—	1	1	—	4	4	2	2	—	14

CLASSIFICATION.

Express Passenger			67
Ordinary Passenger		33	41
Coaches		8	
Fish and Milk		4	78
Express Goods—Class "A."	22	74	
" " Class "B."	52		
Ordinary Goods		55	111
Minerals		46	
Light Engines		10	
Total number of Trains during the 24 hours ...			297
Maximum number of Trains in 1 hour...			16
Minimum " " "			7

TABLE IV.

BASIS OF CALCULATION.

Showing Number of Trains, classified, passing certain points of a 50-mile Section of Line of Railway; calculated Mechanical Horse-power and Electrical power for working the same, together with apportioned speed, load, etc.

Class of Train.	No. of Trains per Day of 24 hours passing.		Basis of Calculation.							Total.	
	Luton.	Harpenden.	Apportioned No. of Trains per hour.	Speed. Miles per hour.	Train Miles per hour.	Load, including Engine. Tons.	Total Tractive Effort per Train. lbs.*	Mechanical H.P. per Train.	Equivalent in K.W. hours.	Mechanical H.P. per hour.	Equivalent in K.W. per hour.
1. Express Passenger	62	67	3	50	150	275	3,575	477	356	1,431	1,068
2. Ordinary Passenger and Empty Coaches	36	41	2	32	64	300	2,130	182	136	364	272
3. Express Goods and Perishables	74	78	4	35	140	400	3,160	295	220	1,180	880
4. Ordinary Goods and Minerals	115	111	5	25	125	500	2,750	183	137	915	685
Totals	287	297	14		479					3,890	2,905
Average per hour	11·9	12·4									

Tractive effort, lbs., per ton $= 3 + \frac{V^2}{250}$ where V = speed in miles per hour. * Tractive effort × load tons = Total tractive effort per train.

$$\text{H.P.} = \frac{\text{Tractive effort lbs.} \times \text{miles per hour.}}{375}$$

The classification adopted is necessarily somewhat arbitrary, but in its construction I have endeavoured to err on

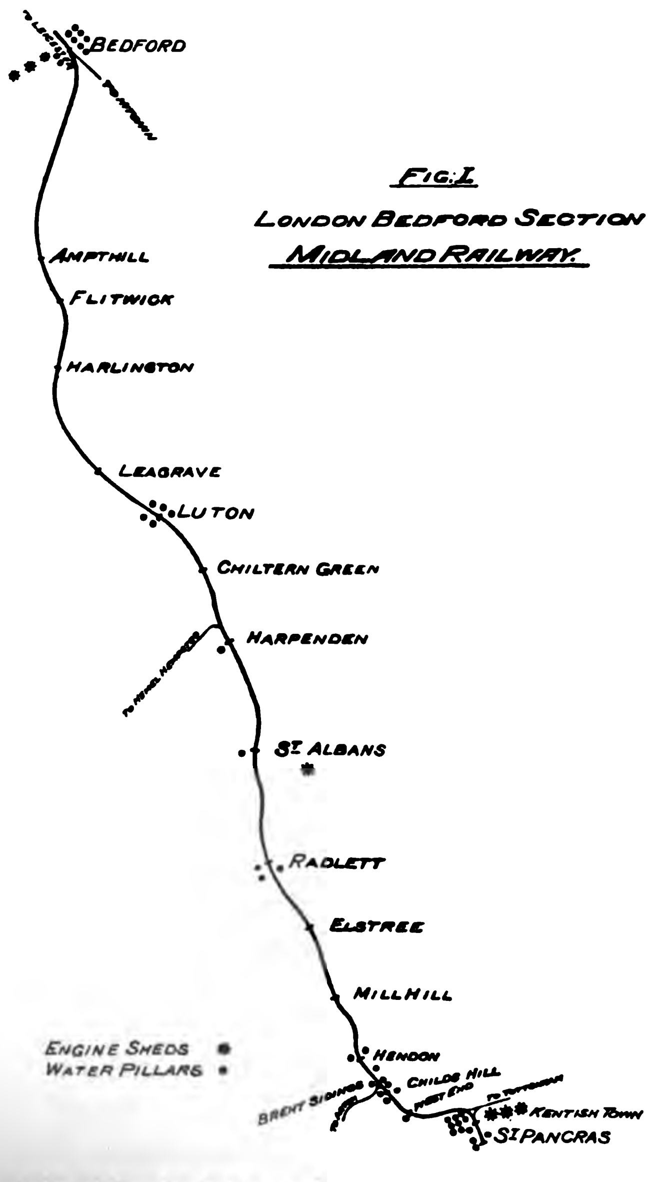


FIG. I
LONDON BEDFORD SECTION
MIDLAND RAILWAY.

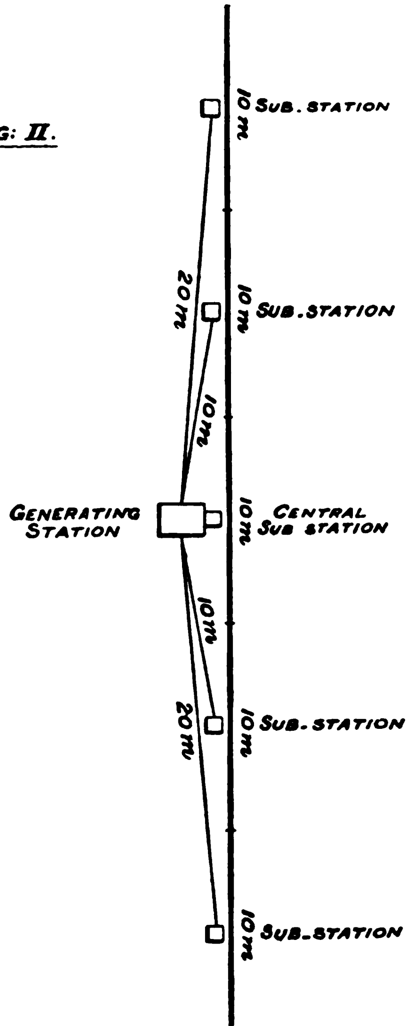
the right side—for instance I have debited each train of its class as a loaded train, whereas some would certainly be

light trains. Again I have, I believe, accorded to each their full merit of speed, although there is reason to assume that, in many instances, certain goods and minerals would not observe that allotted to them. From this table we get the tractive effort per train, and the consequent mechanical and electrical power required to deal with *one hour's work.* The work may, and of course does, vary from one hour to another; indeed, that is clearly shown by Tables II. and III.; for instance, the trains passing Harpenden vary per hour from 7 to 16, and Luton from 7 to 19, but the section of line chosen may be regarded as a full line, a line well occupied by trains both night and day, Sundays and week-days. The returns show an average of 11·9 trains passing Luton and of 12·4 passing Harpenden, per hour. In taking an average, however, I have apportioned no less than 14 trains to the hour. This number divided between the four lines of metals gives a result of 3·5 trains per mile per hour, per line of metals. It may here, however, be as well to point out that, so long as the appropriated number of trains is fairly that in practice, the number applied is, for comparison purposes, immaterial, for the comparison I draw is based upon the train mileage cost.

Fig. 1 illustrates the section of line under consideration. The position of engine-sheds is shown by stars (*); of water-cranes by dots (•). The short or suburban traffic between St. Pancras and Hendon, and the Tottenham line, &c., is not included in my schedules, for the reason that, viewing the section of line as if actually subject to electrical working, there would be little doubt that this immediate London-Suburban traffic would demand a generating station nearer home than that indicated in the scheme I have adopted as my basis for the comparison of costs for working a 50-mile section of line.

The Plant scheme is roughly outlined in Fig. 2. Midway in the 50 miles of railway is the central station, containing four 2,500 kilowatt, three-phase, or other characteristic, 10,000 volt generators. At this pressure current is distributed to substations, each serving 10 miles of railway, where the potential is converted to 600, from whence it is carried to the contact-rail. Or the centre 10-mile section may be provided for by direct-current generators served from the same steam plant.

FIG: II.



The efficiencies of the various parts are assumed as follows :—

	Loss. Per cent.		Efficiency.
Motors	15	...	·85
Ohmic loss in rails ...	10	...	·90
Leakage	$2\frac{1}{2}$	...	·975
Rotary converters ...	10	...	·90
Static transformers ...	7	...	·93
T. transmission ...	10	...	·90

The total kilowatts required on the train-wheels (Table IV.) is 2,905 per hour, and the number of trains is 14. Therefore $\left(\frac{2,905}{14}\right)$ 207·5 will be the average kilowatts per train hour; and, assuming the efficiency of the motors at 85 per cent., $\left(\frac{207·5}{·85}\right)$ 244 kilowatts will be the average power required to be supplied to each train. If now we allow 10 per cent. loss in the rails which supply and return the current, a pressure of 540 only will be available at either end of a section of 10 miles. Therefore the amperes per train that will be required will be $\left(\frac{244 \times 1,000}{540}\right)$ 452.

As there are 14 trains per hour in the total 50 miles section, there will be $\frac{14}{5} = 2·8$, say 3 trains in one section of 10 miles to be supplied by one substation.

$$452 \times 3 = 1,356 \text{ amperes.}$$

Adding 2½ per cent. for leakage, we have 1,390 amperes as the current to be supplied by each substation at 600 volts.

The efficiency of the converters and static transformers is taken at, respectively, 90 per cent. and 93 per cent., and the three-phase transmission at 90 per cent.

$\therefore \frac{1,390 \times 600}{1,000 \times ·9 \times ·93 \times ·9} = 1,107$ k.w. to be delivered to the trains for each section containing 3 trains.

As there are 14 trains, there will be 4 sections with 3 each, and 1 section with 2 trains.

Now the 4 sections with 3 trains each would require—

$$1,107 \times 4 = 4,428 \text{ kilowatts.}$$

To allow for the maximum loss let it be assumed that the centre 10-mile section, which as previously indicated might be supplied direct at 600 volts, is so dealt with, and that it is the section that has two trains only.

The kilowatts required would equal—

$$\frac{1,390 \times 600}{1,000} \times \frac{2}{3} = 556 \text{ kilowatts.}$$

Therefore the total kilowatts to be generated would be—

$$(4{,}428 + 556) = 4{,}984, \text{ say } 5{,}000,$$

and the combined efficiency $\left(\frac{2{,}905}{4{,}984}\right)$ would be 58·3 per cent.

This 5,000 kilowatts is the power required to be generated to work the 14 trains travelling 479 miles during the hour, as shown in Table IV., and upon it all comparative calculations and deductions have to be based.

The demand may, of course, go beyond this, or it may be less, but so long as the generating power, and that of the corresponding parts, is there to meet it, that branch of the question may be disregarded. As I have previously stated, the comparison is made between the ascertained quantities travelling a stated mileage at a stated speed. If the mileage were greater, or the number of trains greater, the comparison would be equally applicable. It is, in respect of the mileage result, immaterial, so long as we apply it to a fairly representative condition. The speed and the train load indicated in Table IV. will, I believe, bearing in mind that the number of trains are regarded as all carrying the full load indicated, be found to conform to this.

A generating plant capable of an output of 10,000 kilowatts is, of course, ample to provide for a calculated demand of just half that amount. I adopt the following estimate as applicable to the prime cost :—

Generating Station.

Buildings, foundations, chimney stacks, &c.	£50,000	
Equipment, including steam units, boilers, coal conveyers, steam mains, condensers, pumps, &c.	200,000	
		£250,000

Substations.

Buildings—5 stations	10,000	
Equipment of ditto with transformers, converters, &c., delivering at 600 volts, and all necessary fittings ...	70,000	
		£80,000

Cables, including laying	£70,000	
Contact rail—200 miles (*i.e.*, 4 roads of 50 miles each)	70,000	
		140,000
Total capital outlay for generation and distribution of current		£470,000

The figures comprised within the two first items are practically those given by Mr. Parshall with reference to a somewhat similar plant, with the exception that the amounts have been increased to the extent of £15,000 for the generating station equipment, consequent upon the adoption of a higher voltage; and £10,000 for generating station buildings to meet any advance of prices. £10,000 has also been added for independent transformer buildings. The cost of cables, including laying in position, is based upon the current demand and mileage.

The hourly demand has been shown to be 5,000 kilowatts. The annual output for the section of line under consideration will therefore be this amount multiplied by the twenty-four hours per day, and 365 days to the year, viz., 43,800,000 kilowatt hours.

On this basis I estimate the annual cost for generation and distribution of current as shown in the annual column of the following table, from which, by dividing the sums there shown by the annual kilowatt output, I obtain the per kilowatt-hour charge.

To the foregoing we have to add the cost for drivers and assistants in attendance upon the electric locomotives—performing practically the same duties as the driver and firemen in attendance upon the steam locomotive; together with that for repair and renewal of all machinery, including the locomotives.

In his presidential address to the members of the Institute of Mechanical Engineers in 1898, Mr. S. W. Johnson, the Locomotive Engineer for the Midland Railway, furnished valuable data in relation to the cost of moving railway trains. From this data I, by his courtesy, am enabled to furnish the details shown on Table VI.

TABLE V.

ESTIMATED COST OF GENERATION AND DISTRIBUTION OF CURRENT.

[Output, 43,800,000 kilowatt hours.]

Details of Charges.		Per Annum. £	Per Kilowatt hour. Pence.
1. Capital outlay, £470,000. Interest at 3½ per cent.		16,450	0·0901
2. *Generating Station.*			
Salaries and Wages :—	£		
1 Chief Engineer	500		
1 Assistant ditto	250		
3 Switchroom attendants, at £150 each	450		
1 Clerk	120		
7 Engine-room attendants, at 40s. each per week... ...	728		
7 Assistant ditto, at 35s. each per week	637		
12 Stokers, at 30s. each per week	936		
15 Labourers and Cleaners, at 22s. each per week ...	858		
		4,479	0·0245
Coal.			
At 3·0 lbs. of coal per kilowatt hour — 58·660 tons, at 7s. 11½d. per ton ...		23,345	0·1279
Water.			
At 25 lbs. per kilowatt hour, and 2d. per 1,000 gallons		913	0·0050
		28,737	0·1574
3. *Substations* (5).			
Salaries and Wages :—	£		
5 Assistant Engineers, at £200 each	1,000		
20 Attendants, at 40s. each per week	2,080		
20 Assistant ditto, at 35s. each per week	1,820		
10 Cleaners, at 22s. each per week	572		
		5,472	0·0299
4. *Outdoor Service.*	£		
5 Rail-jointers and Fitters at 40s. each per week	520		
Material, &c.	230		
		750	0·0041
5. Oil, Waste, and Sundries		2,000	0·0109
Total estimated cost of Generation and Distribution of Current		£36,959	0·2023

[illegible]

[illegible]	£	Pence [illegible]
Running Expenditure.		
Wages—Drivers and Firemen	[illegible]	[illegible]
Cleaners, [illegible] &c.	[illegible]	[illegible]
Water	[illegible]	[illegible]
Oil and Stores	[illegible]	[illegible]
Coal and Coke	[illegible]	[illegible]
Total Running Expenditure	[illegible]	[illegible]
Repairs and Renewals.		
Wages	[illegible]	[illegible]
Materials	[illegible]	[illegible]
Total Repairs and Renewals	[illegible]	[illegible]
Salaries	[illegible]	[illegible]
Turntables and Buildings	[illegible]	[illegible]
Gas	[illegible]	[illegible]
Gross Expenditure	[illegible]	[illegible]

Tons of Coal and Coke consumed	[illegible]
Cost per Ton	[illegible]
Train Mileage	[illegible]

This table shows that the average cost for twenty-four years, for drivers and firemen was £358,635, or 2·650 pence per train mile; and that for repairs and renewals, £357,400, or 2·641 pence per train mile.

To arrive at the cost for drivers and attendants for the electrical locomotive, I might revert to Table IV. and deduce from it, at a given rate of wage per hour, for the number of trains occupying the line for that period, the kilowatt-hour cost, but this, it appears to me, would not be quite right. There can be no question that the cost incurred by the locomotive department is extremely heavy,[1] but it is a

[1] Assuming the weekly wage of driver and fireman to amount to ninety shillings, probably an excessive sum, it would seem that the weekly mileage travelled would be but 407. If the wage were seventy shillings, the mileage would be but 317.

charge incumbent upon the working of the traffic, and whatever are the conditions which militate against a reduction in this charge with the locomotive department, presumably they would hold good against the electric unless the traffic could, under the latter, be so facilitated as to enable it to be got through with greater speed, and less shunting. It is not clear this could be done. I am therefore, very reluctantly I must admit, obliged to adopt the extremely heavy cost incurred by the steam locomotive, for there would be very little, if any, difference in the rate of pay to the respective class of men.

The repair and renewal of electrical machinery, whether in relation to the generating or the locomotive plant, should be considerably less than that of the steam locomotive plant, for the reason that there will be extremely few moving parts, while many small units, used for pumping and other like purposes, would be provided for from the central generating station at a less cost, or entirely abolished.

The train mileage (Table IV.), run by the fourteen trains during one hour, is 479. Assuming that the cost attending the repair and renewal of the electrical machinery will be 2d. per train mile, as against that for the steam locomotive power and works, viz., 2·641, we shall have a result of 0·1916 pence per kilowatt hour.

There is yet one more addition to make. The cables and contact-rail are peculiar to the electrical system. I do not include them in the above 2d. per train mile for repair and renewal of machinery. Having regard to the value of the recovered material, I assume 2½ per cent. on the primary outlay will meet the renewal of cables, and 4 per cent. that of the contact-rail. This means 0·0249 pence per kilowatt hour.

Table VII. furnishes all these items, against each of which is also shown the cost per train mile, *i.e.*, the cost per kilowatt hour multiplied by the total output, viz., 5,000 kilowatts for the hour's work, divided by the train mileage worked during the hour, viz., 479.

TABLE VII.

ELECTRICAL CHARGES.

Cost in Pence, per Kilowatt Hour, and per Train Mile.

	Per Kilowatt Hour.	Per Train Mile.
Generating charges	0·1574	1·643
Sub-stations charges	0·0299	0·312
Out-door attendance	0·0041	0·043
Oil, waste, and sundries	0·0109	0·114
Locomotive drivers and assistants. [This item is shown at the cost incurred under present mode of working]	0·2538	2·650
Repair and renewal of machinery, motors, &c. ...	0·1916	2·000
Renewal of cables and contact-rail	0·0249	0·259
Total cost for power and haulage	0·6726	7·021
Interest at 3½ per cent. on primary outlay, viz., £470,000	0·0901	0·941

Therefore, if my deductions are correct, it would appear that the cost of working by electricity as against that for the steam locomotive is, per train-mile, so far as the Midland is concerned, as 7·021 to 8·943 pence, being an apparent saving of 1·922 pence per train-mile, or £260,155 on the average yearly cost for the 24 years indicated.

A closer comparison of the chief items may help us to learn where and how so large a saving is effected.

Coal stands in Mr. Johnson's data at 727,889 tons, at a cost of £289,595. Based upon the figure adopted by me, viz., 3·0 lbs. per kilowatt hour, the tonnage required is 454,145,[1] and the money £180,712.[2] The steam loco-

[1] $\frac{5{,}000 \text{ K.W.} \times 3{\cdot}0}{479} = 31{\cdot}315 \quad \frac{32{,}485{,}530 \times 31{\cdot}315}{2{,}240} = 454{,}145$ tons.

[2] $\frac{{\cdot}1279^{d} \times 5{,}000 \text{ K.W.}}{479} = 1{\cdot}335073^{d}$ per T.M.

$32{,}485{,}530 \text{ T.M.} \times 1{\cdot}335^{d} = £180{,}712.$

motive consumes on the average 50·191 lbs. of coal per train mile. My figures place the quantity required for electrical energy at $\frac{(5{,}000 \times 3{\cdot}0)}{479}$ 31·315 lbs. The saving under this head is, therefore, 273,744 tons, which at 7s. 11½d. will account for £108,927. We have to bear in mind that the calculation on which the cost of electricity is based makes no provision for shunting operations. It is based entirely upon the train mileage run. Shunting work is, of course, included in Mr. Johnson's figures, and will account, to some extent, for the difference. The main gain, however, is to be found in the economy of a stationary, as against an itinerant generator, as well as in the fact that much coal is consumed by goods and mineral trains when shunted, and by all trains when standing at stations, the whole of which would be saved if worked by electricity.

With stationary engines a less expensive coal than that used for locomotives would be available, thereby effecting a reduction probably more than sufficient to meet the cost for shunting previously alluded to. It may be pointed out that coal is more costly at present. Such is the case, and, were my calculations based upon the present rate of coal, the result would largely enhance the advantage of electricity. Say, for instance, that coal stood at 10s. instead of 7s. 11½d., the locomotive cost would be 727,889 tons at 10s. = £363,944 instead of £289,595. That for electricity would be 454,145 tons at 10s. = £227,072, instead of £180,701. The result would be that electricity would show, under coal at 10s. a ton, a saving of £136,872 instead of £108,927, on precisely the same mileage, with a proportionate increased saving at prices ranging above that figure.

The fact that dear coal enhances the comparative value of an electrical system—especially with the possibility of coal at a higher rate than 7s. 11½d.—cannot be too strongly emphasised.

Water.—The steam locomotive calls for £28,573, or 0·211 pence per train mile; electricity, £7,066, or ·0522 per train mile—a difference in favour of the latter of £11,507. It is difficult to attempt a comparison between the cost of a largely-scattered supply—water pillars at numerous stations —and a concentrated one—one to every 50 miles or so of line. The site for such a generating station would naturally be

selected with a view to cheap water supply, and, as a rule, no great difficulty would attend its selection. A further point to be borne in mind is that, naturally, all machinery would be of the most modern and economical type, and that the working would reach the highest ideal for an electrical plant, viz., an actually constant and perpetual demand.

Drivers.—It is quite clear that whatever may be the cause of the existing heavy charges, it will apply equally to electric as to steam locomotives. The engine must stand by its train, and the men along with it. Any reduction that may be effected will be in the mode of dealing with the traffic. The present condition, viz., that of a mixed traffic travelling at various rates of speed—one class of train being required to make room for another of a more important character—is not destined to effect economy in this branch of expense.

Repair and Renewal.—I have assumed that the cost of repair and renewal would be practically $\frac{1}{3}$th less with electricity than steam. I think I am more than justified in this. The wear and tear of stationary engines, or motors, cannot possibly be so great as that of the steam locomotive. The number of electric locomotives would necessarily be as great, but their wearing parts would be immensely less, than those of the latter. Many local units would be entirely dispensed with.

Oil and Stores forms a somewhat large item in the running expenses of the steam locomotive. Much of this is for the lubrication of moving parts which would be non-existent in electric engines. Moreover, with stationary engines it is possible to recover, and again use, a great portion of the lubricator employed. It would appear that considerable economies in oil should accompany the employment of electricity.

We may now perhaps glance at possible economies on that which is indispensable for the steam locomotive, but which is unnecessary, or not so largely necessary, with electricity. Water pillars, turntables, engine sheds, coal stages—all these are expensive items which with electricity are either not required, or capable of considerable modification.

Water pillars, supplemented in many instances by fixed engine-power for pumping the water into reserve tanks, involving power-houses, sheds, and other structures, form part of all large stations and many other points at which the locomotive requires to take water. With electricity, water to any extent would be required only at that point at which the central generating station is placed. If this station served fifty miles of railway, then it would take the place of all the pumping plants, water pillars, &c., otherwise required throughout that section for the steam locomotive. Every pumping station involves the provision of labour, fuel, &c. All water pillars require special attention during hard weather. Economies in first outlay and annual charges on this account should accompany the use of electricity.

Turntables would be unnecessary. The annual outlay for repairs in this respect is not great, but the cost of laying down the large turntables now required is very heavy.

As the number of locomotives increase, so increases the demand for engine sheds. Electric locomotives would of course need housing as well as the steam locomotive, but the space which they would occupy would probably be about half that now required.

Wherever we find an engine shed, there we see an area of land covered with coal; lines of rails applicable thereto; coal-stages to which the coal has to be carried, and from which it has to be distributed to the locomotives. First the coal has to be stacked, then loaded into trucks and carried to the coal-stage; and thence weighed and placed on the engine's tender. This is the course of procedure at each engine depôt. If we compare it with the work of a large central station, such as that sketched out in this paper, I think it will come home to us that although the work would be large, it could not be nearly so large as at present. If stacking were at all necessary there would be the less quantity to stack, and it would all be dealt with in a more concrete form and at comparatively few centres. Again, in this respect there is reason to look for economies in land, in buildings, and labour.

All these are assets directly due to the employment of electricity. Others, not considered in the figures which I have advanced, would, with its presence, be available: the of the trains, stations, goods warehouses and yards,

marshalling grounds, &c. Signalling, to some extent, might become automatic; while at large centres where signal boxes have become both numerous and cumbrous, it would appear but reasonable that, with the aid of such a power, points and signals might not only be actuated, but the means for operating them might be so condensed as to admit of the entire duties being embraced within such a space as would enable one man to deal with them. Labour and space would be economised, and less time would be occupied in giving effect to the various operations than is possible with the existing means. To this we may add that obviously it would also provide for the operation of lifts, and other local demands for power which at present have to be met by isolated plants.

Let us now turn to what may perhaps be regarded as difficulties to be encountered.

The first question that will arise will be :—Are we safe in placing so many of our eggs in one basket? With the steam locomotive we have a travelling unit which has to manipulate its own load and is in no way responsible for the duties of others. If it breaks down, the inconvenience is chiefly confined to the vehicles it is hauling, and in due course they are extricated from their difficulty by the aid of another engine.

With electricity we are locating our power at one spot in so many miles of line, and if that breaks down that section of line is practically dead. But with the usual spare parts —the duplication, if necessary, of the generating units— there should be no reason to anticipate such a failure. The same argument in a measure applies to the power at the distributing stations. Here, however, the case may be met not only by duplication, but by, in emergency, connecting one section through to its neighbour. For the time being inconvenience would be felt; speed would be reduced, but traffic would not be stopped. Of all this we have evidence in that which has already been done. We see railways being worked, tramways operated, and other large undertakings all dependent upon one large and central source for their life and being. Electric railways have become an accomplished fact, and we may turn to those that are in use as exponents of success or of difficulties to be encountered.

Does the magnitude of the question we are considering —the eventual supersession of the steam by the electric locomotive—remove it from the category of that which has so far been done? I think not. The basis is there. It is to-day in useful operation. Improvements will come. The mode of working which we see to-day may, and probably will, be simplified, but this will only strengthen that which has already been accomplished.

Still, there are points of great interest for consideration. As a rule, that which has been done has, with one or two exceptions, been confined to underground lines, and these exceptions have not as a rule dealt with such heavy work as the large overland railways require to compass. Overground railways have to work through all kinds of weather—rain, snow, fog—and at times to pass over rails submerged in water. Winter floods are not unusual in certain localities of nearly all overland railways. These are conditions which will affect the construction of the locomotive, the arrangement of the current collector, and the contact-rail, and are subjects for thought not only in themselves but in relation to the mass of under-gear which now appertains to all passenger railway stock, as also to the relative position and construction of roads, their repair and renewal.

A question may here intervene whether a similar economy would attend the operation of small branch lines of railway where the trains are few and far between. Consider! Why are the trains so few and far between? The traffic is, we will say, a fixed quantity. There are only so many tons of goods and so many passengers passing over it daily. The steam locomotive is available only at certain times, and to attain economy it is necessary that so many trips only should be made. The accommodation is limited to this. But if the power for working the trains were constant, although reduced, any number of trips might be made. The additional cost would be that only of the driver, for the rest the branch would be no more costly, while the frequency of the communication would tend towards the development of the district, and the consequent increase of trade.

I have now, I believe, fairly set forth the salient features of this question. The data which I have produced speaks greatly in favour of electrical energy as a motive power for the movement of railway trains. The economies which it would apparently effect are, indeed, so large as to raise a doubt whether my deductions—whether the figures I have adopted—are fair and reasonable. It must not be forgotten that my calculations are based upon a mileage run clear of stoppages or other contingencies. Stoppages are unavoidable. They *will* arise, and provision must be made to meet them. But I find it difficult to identify them further than I have already done. Stoppages will not affect the coal bill. I have taken the present cost of drivers and firemen to apply to that for drivers and assistants for electric locomotives. My allowance for repair and renewal of machinery will, I think, be generally supported. Necessarily the subject has to some extent had to be dealt with more in the abstract than in detail, but I venture to hope my figures will not be found illiberal. We must not lose sight of the fact that the conditions are a constant load and continuous output for every hour of the year. I believe the cost of shunting at stations and in goods yards may be met by the economy attending the use of a cheaper coal than that which is necessary for the steam locomotive; but assuming that some provision should be made for this, for administration and contingencies, I conclude that 20 per cent. (say £50,000) of the accredited saving will cover it.

We thus bring the net annual advantage to approximately £208,124, and if we deduct interest on the primary outlay it will further reduce it to £191,674. Whether it is, in face of the savings to be affected in engine-sheds, coal-stages, water-cranes, &c., fair to make a debit in full of this amount, I must leave those who are interested in the question to determine. Broadly, it appears to me to mean this: that were a new company to start with electricity as their motive-power, they would not need to take into consideration the interest on the entire additional outlay, because they would save a great portion of it in other directions.

But were an established company to adopt it, they would already have incurred the cost for the lands, buildings, &c., and the expense for establishing electric working would

unavoidably prove to them, for some years, an addition to their capital charges.

Although I feel that my reason for pursuing such a course will be obvious, it may be desirable that I should emphasise the fact that my sole object in availing myself of the data afforded by Mr. Johnson's presidential address to the members of the Institution of Mechanical Engineers is that I might deal with data extending over a long period of years, rather than draw a comparison with a period which might be regarded as possessing some abnormal feature. It must, however, be noted that recent figures tend to greatly magnify the result. I have shown that, with electrical working, certain economies are to be anticipated. The annual amount of these economies is based upon the ascertained saving *per train mile*, multiplied by the average annual mileage for the twenty-four years—viz., 32,485,580. The mileage for 1899 is 45,453,438, and the expenditure £2,006,069, as against the twenty-four years' average £1,122,899; and the train-mile cost has risen to 10·59 as against 8·943 pence. Assuming that the saving per train mile remained the same—it would probably be larger, consequent upon the increased cost of coal—the resultant saving would be, independent of any deductions for interest or contingencies, £364,006.

Large as is the apparent economy thus presented, it would, were it possible to employ a gas plant for so large an output, be increased by the use of the "Mond" gas system. Such a system would appear to invite consideration, at all events, for smaller installations, as, for instance, for the operation of branch lines.

Here it may perhaps be asked: What good can attend the production of these figures, or the results they advocate, seeing that the railway system generally is wedded to the steam locomotive? Is it probable that any railway company will cast on one side their present equipment for the purpose of taking up that, which, although holding out fair hopes of a large economy, is yet, in a measure, or in the large measure to which it would have to be applied, mainly an untried agent? To this I reply: Railway companies are under the direction of business men—men who know the value of money. Satisfy them that economies are to be ef[illegible] 'o not let us forget that the economies are

not confined solely to those with which this paper has dealt, but probably to many others indirectly associated therewith—and that the economy embraces a reliable means of working, and they will not be found undesirous of testing its worth. The life of a steam locomotive is not an indefinite quantity. Its replacement by one of more modern construction or of greater power, quite apart from its ultimate destruction by wear and tear, is an appreciable fact. As traffic increases, so additions have to be made. We see so many new engines ordered year by year. What is to prevent a railway company, instead of thus perpetuating its annual costs, setting apart portion by portion of their system for operation by electrical energy, and, instead of ordering for, say, that portion of their system, steam, to order electric locomotives; and thus to bring, piece by piece, their entire system under electrical operation? No sane railway management would do otherwise; and I assume that, should my figures stand unrefuted, no railway company would desire to follow any other course. Prove its economy, prove its reliability, and there is nothing in the fact that railways are, for the time being, the slave of the steam locomotive, to militate against their supersession by electrical energy.

That this paper deals with a problem which, sooner or later, will force itself upon the attention of all who are interested in railway progress, few will be disposed to dispute. So far it has been considered solely in the realm of economical working—in the interest of the railway shareholder. But are we justified in looking at it only from this standpoint? Does it not embrace a question of still greater magnitude? Is it not one of even national interest? If, by the aid of electricity, we can save no less than 18·876 lbs. of coal per train mile, it is clear we could save *no less than three million tons* a year [1] if all our railways were worked by that agency.

View it again from still another standpoint. Twenty thousand locomotives moving about throughout the land cannot fail to leave their mark behind them. Our railway

[1] 396,241,265 train miles × 18·876 lb. = 3,339,040 tons.

stations, the telegraphs which traverse the railway routes, the trees which grow on its borders, all bear evidence of their presence.

The supersession of the steam locomotive by the electric locomotive will bring with it a purer and a more cleanly atmosphere—cleanly railway stations—cleanly railway carriages—a higher and a purer sanitary condition of life.

Mr. Robinson.

Mr. MARK ROBINSON: I am sure I shall not be accused of wasting the time of the meeting in compliments if I begin by expressing the view, which I believe all present hold, that this is a most interesting and valuable paper. It deals with a matter of the utmost importance to the country and to its industries. It is one we are all anxious to hear about, and it has been put before us in the most practical manner, by dealing with a concrete case. Mr. Langdon's conclusions, even if we think some deductions should be made from them, cannot fail to be welcome to many in this room. He has not spoken as an enthusiast—scarcely as an advocate—and he has treated the subject with complete impartiality and moderation. In fact his almost excessive moderation is my excuse for attempting to criticise him, and for endeavouring to show that he has not made the best case for electricity which the circumstances, as given in his paper, admit of. Mr. Langdon proposes five sub-stations, each feeding ten miles of line, with a continuous current of 600 volts. I have no wish, and do not propose, to question his scheme in these leading features: I only hope to show that, these being granted and his estimates assumed correct, a different arrangement of plant would lead to a great reduction in first cost as well as in working expenses. If his estimates are too high or too low, that does not affect my argument, which relates only to comparative results, and might be based equally well upon any other figures. What I question is the need for the big central station which supplies the sub-stations, for I believe that 2,000 kilowatts could be generated in each of the five stations as economically as 10,000 kilowatts could be generated in one large central station, or if not as economically, then so nearly as economically that there would still be great advantage in working by the more numerous and smaller stations. The 10,000 kilowatts correspond to about 16,000 horse-power, or say four engines of 4,000 H.P. each. It is commonly believed that as engines attain to very large sizes they become much more economical, and it is probably Mr. Langdon's view that there will be such notable savings in coal and in other things as to justify the great cost of the cables leading to the sub-stations and the very serious losses which he foresees in transformation. I venture to assert that there is no such great economy in increasing the size of the engines. It is reasonable to suppose that Mr. Langdon has turned for inspiration to the country where electric traction has received its greatest development, namely, America, where they believe very much in large stations. But in America only the slow-running engine is present to the minds of their engineers, and of such engines it is true

Mr. Robinson.

that economy improves with increase of size. It ought to do so, because the relation between the exposed surface in the cylinders (which is the immediate cause of condensation, the principal heat loss we have to guard against) and the volume of steam which the cylinders contain becomes more favourable as the size of the cylinder increases. Really high-speed engines, which are chiefly used in this country and are practically unknown elsewhere, stand, however, upon quite a different footing. In them a new factor tending to economy comes into play, which has practically no existence in any slow-running engine, namely, the great shortening of the period during which the surfaces are exposed to the successive alternations of temperature. That is undoubtedly the chief reason for the remarkable economy of the English fast-running engines even down to very small sizes. In such engines, when you decrease the size, you are able still further to increase the speed and to further reduce the time of exposure of the surfaces, so as to more or less completely compensate for the increased condensation due to the worse ratio between the surfaces and the volume of the cylinder. In a slow-running engine the speed may also be increased as the size diminishes, but within far smaller limits, and the speed attainable with this type of engine, however small the engine may be, is in no case sufficient to act materially upon economy. These facts, though apparently ignored out of this country, are established by trials and records well known to the scientific world, and I would apologise for bringing them forward in discussing a subject apparently remote from the merits of rival types of engines, if it were not that the capacity of one type, and of one type only, to give very economical results in moderate sizes goes really to the root of the question. Those who remember the remarkable figures obtained in the well-known experiments of the late Mr. Willans, may have forgotten that the engine on which he tried those experiments, and with which he obtained a consumption of only 12½ lbs. of steam per indicated horse-power, was of 40 H.P. only. On the score, therefore, of steam economy there is no necessity to use very large engines, or to collect all the power in one great station; hence the main (high-tension) cables may be left out, and a great loss of energy, as well as first cost, avoided. To go to figures, I would first say that I have consulted several electrical engineers who have had experience of stations large and small, and they support my belief that stations of about 3,000 H.P. (the size of Mr. Langdon's substations) can be run at almost, if not quite, the same cost per kilowatt as a station very much larger. Each of the five stations would be a little smaller than I could wish; possibly four would give a better result, but even the suggested stations of 2,000 kilowatts each would contain three engines of 1,000 H.P. each, or four of 750, and I venture to say either of these would use as little steam per kilowatt as engines of any size or type. They would certainly cost less for attendance, and the office management expenses would in my opinion be no greater. Five such stations on the same line of railway, and connected by telephone, would be practically one concern, each separate station being merely a foreman's job. The several stations would assist each

Mr. Robinson.

other with current, and I should not anticipate any excessive changes of load in the separate stations as the trains passed from one section to another. Only this week I have been engaged in designing a station which is to have some 10,000 H.P. in it, and it was actually found desirable for convenience, and for safety against breakdowns, to divide this large power into groups of plant, of course in the same engine-room. If those groups stood ten miles apart (all on sidings on the same railway) I do not think the inconvenience would be great. Assume that in each of the ten-mile sections a site can be found with good water supply, which perhaps is a large assumption, and that we install one-fifth of the power there, and that all the stations, instead of one only, deliver continuous current direct to the rails. Let us assume that the five stations cost as much to build as the one big one (priced at £50,000 in the table on page 136), plus the £10,000 which has been allowed for the five sub-stations; they will then cost £60,000. I think that is a safe estimate, because very large stations increase rapidly in cost, owing to the great height of the buildings, the large span of the roofs, and the great power and weight of the travellers required. Assuming also that the £200,000 provided for generating plant is available and is sufficient, as I believe it would be, for the five smaller stations, then we should at least save the £70,000 allowed for the equipment of the sub-stations, for the transformers, and so on, and we should further save the £70,000 put down for cables. That is £140,000 out of £470,000, and it brings our first cost down to £330,000. But in addition we should wholly save the 17 per cent. which is given us, at page 134, as the loss of energy in the rotary converters and the static transformers, and we further save the 10 per cent. lost in the high-tension transmission. Hence the 1,107 kilowatts required (page 135) to be delivered to each section containing three trains, is reduced to 834 kilowatts, and the total is reduced from 4,984 to 3,892 kilowatts, a reduction of no less than 22 per cent., involving also a reduction of 22 per cent. off the £260,000 we have allowed for the buildings and plant. This amounts to over £57,000, so that we really bring down this £470,000 capital charge to about £273,000, and we may hope to save the same 22 per cent. upon the cost of running the plant. I am prepared to learn that there may be difficulties in finding five sites for stations conveniently placed on these five sections, with good water supply, so that either from a reduction in the number of stations or from some being disadvantageously placed, something would have to be given back again in the form of increased cost for conductors; but the total saving at our disposal is really so great that we can afford to deduct a good deal from it and yet show a very large gain. On railways with a very light traffic spread over a long distance, or on Metropolitan lines where there would be a difficulty in getting sites for stations, high-tension transmission with transformers at the end may be perfectly right, but for railways in general that system ought to be the exception rather than the rule. There is another economy which Mr. Langdon, I am glad to see, has in reserve, namely, that when a railway becomes wholly electrical the passenger trains may be run upon the better system of having the motors on the axles of the carriages, which for passenger trains gives many advantages, and is

less hard upon the permanent way. I can only thank Mr. Langdon again for having given us such a very fruitful paper, and a paper which should set us all thinking. Mr. Robinson.

Mr. G. C. CUNNINGHAM: The extremely interesting and valuable paper that Mr. Langdon has read opens up, I think, a very fruitful subject for discussion. The question of the application of electricity to locomotion, to my mind, largely turns upon the question of the pounds of coal per ton mile. Some years ago I wrote a short paper, that was published in the Proceedings of the Institution of Civil Engineers, upon the subject of the consumption of fuel in locomotive engines on railways. That was at a time when I was in Canada as chief engineer of the Canada Southern Railway. This railway line is extremely level, with very flat gradients—there are none more than fifteen feet to the mile—and it is almost entirely free from curves. There is one long straight section of fifty-three miles, joined by another long straight portion of fifty-four miles. Hence this line was perhaps the very best possible for doing work with a small consumption of fuel. The goods trains on that line were therefore long, and were drawn by one engine. The consumption of fuel by these goods trains worked out at 0·15 of a pound of coal per ton mile. On the passenger trains, where a much higher speed was obtained, the consumption of coal was 0·8 of a pound per ton mile. Now, of course, electrical railways present precisely the same problem, the gradients of the line affecting the amount of power used. The quantity of power is dependent upon the resistance of the line—that is mainly on the gradients of the line. On the Liverpool Overhead Railway, which is practically a level line, I found in a paper, which was read before the Institution of Civil Engineers some few years ago, that the quantity of coal burnt per ton mile was 0·4 of a pound. On the Central London Railway, of which we have had a brief experience—I hope you have all had experience of it—the quantity of coal, irrespective of that which is consumed for station lighting and lifts, per ton mile consumed in the power-house, covering all losses in transmission and transformation and so forth, is 0·5 of a pound, as near as I can get it at present, per ton mile. Of course this is, as I say, dependent upon the amount of power used, and the amount of power is again a question of gradient. In Mr. Langdon's very interesting paper the question of power seems to have been assumed at some standard amount, irrespective of the gradients. The quantity of power used on such a line as the Central London, irrespective of the lighting and lifts, is something like 70 watt-hours per ton mile. But, as showing how very much this power varies with the gradients, I may say that on the Montreal Electric Railway, a line running through the streets, of which railway I was the general manager for some years, the quantity of power used was something like 300 watt-hours per ton mile. There, of course, the gradients were very severe, in some cases as steep as 1 in 10 up steep streets. It is therefore difficult *a priori*, until we know the character of a railway, to say how much power would be used on it. Of course, the cost of producing the power again depends upon the character of the power-house in which it is produced. In my own experience the lowest consumption of coal that I have known in pro- Mr. Cunningham.

Mr. Cunningham. ducing a kilowatt-hour is 3·6 pounds, and the lowest cost (*i.e.*, shop-cost) that I have known is something slightly over a farthing, irrespective of depreciation or interest on capital. But that I conceive is a low cost, and I do not know of any case in which it has been further reduced. The question of the application of electricity to railways is one, the development of which would be watched with very great interest by all those associated with electrical work. But I think it is a question that depends almost entirely on the construction of economical power-houses and the economical distribution of the electrical current.

Mr. Parsons. Mr. C. A. PARSONS: I think we are very much indebted to Mr. Langdon for bringing forward this most interesting and important subject at the present time. I may say that it seems to me that the subject has been treated on a thorough and systematic basis, and that the figures he has placed before us (as far as I can judge) err, if anything, on the liberal side. The case might, I think, have been made out more favourable to the electrical running of trains, but Mr. Langdon has thought it wiser to allow a considerable margin. In the cost of generation of electricity, for instance, I think his figure of 3 lbs. of coal per kilowatt-hour errs on the liberal side, in view of the large scale of the generating plant. The question of generating from one central station rather than from a number of small stations connected by telephone, to which Mr. Robinson has alluded, is, I think, scarcely the point before the meeting this evening, but is more of the nature of a side issue. The broad and important question is rather whether trains can in general be propelled electrically on a large scale at anything like the cost of the steam locomotive; and this, I think, has been conclusively shown to be the case by Mr. Langdon. It further seems to me that when the electrical system is worked out in practice there will manifest themselves many and unexpected economies which at the present time cannot be realised. The one great advantage of the large central stations and of high-tension, long-distance transmission, is, of course, the liberty which it affords to the designers of placing the stations in situations where there is practically free water in unlimited amount for condensing and also easy access for coal, and suitable places for the artisans to live. There is no doubt, from the experience I have had, that the larger the station the cheaper it can be run, in consequence of the greater developments in organisation, management, and machinery which are economically possible in an undertaking of the greater magnitude. I think we are very much indebted to Mr. Langdon, and I may, perhaps, repeat that I consider the paper one of the utmost value and importance, and that it may assist towards the initiation of large developments in the application of electricity to the driving of trains.

Mr. Hoy. Mr. H. A. HOY: I have not had time to prepare many notes on Mr. Langdon's interesting paper, but I may say at once that I do not propose questioning any figures which the author has collected from Mr. Johnson's very able paper read before the Institution of Mechanical Engineers. I think locomotive men are practically unanimous with regard to any question that may arise out of those figures, but the little

study which I have given to the question of electric traction on railways leads me to fall in with the views which have been expressed by Mr. Mark Robinson with regard to the use of a direct current at say 600 volts being fed to the main line in sections, and by smaller units than has been suggested by Mr. Langdon in his paper. I fail to see why any resort should be made to the use of large power-stations at infrequent intervals, entailing the use of static and rotary transformers, although the efficiency of those transformers is as high as it can be hoped to expect of them. As Mr. Langdon points out, the loss of efficiency is very small, especially with the static transformers; yet to this must be added the loss of efficiency in one or more rotary transformers, which by reason of them being so made are necessarily less efficient. I suggest that the whole of this be saved by serving sections of railway with direct current from generating stations of a capacity suitable for the work to be done. Most of the members are aware of what Colonel Heft has done upon the New York, New Haven, and Hartford Railway of America. No attempt has been made there to introduce anything beyond what I have named, and the information that I obtained on the ground was to the effect that any extension would follow on the same lines. I believe if a current was fed in the way indicated, a greater efficiency would result than that which is suggested in the paper. One reason lies in the fact that by relying upon large central stations at infrequent intervals there is not so much scope for taking advantage of suitable sites for obtaining water for condensing purposes, which is a very important point, as indicated by Mr. Parsons. The position of the central station as shown in Mr. Langdon's paper, viz., at Harpenden, about twenty-five miles from London, is not, so far as my knowledge of the geography of that part of the country extends, a suitable place for obtaining a large continuous supply of water for condensing purposes. It has occurred to me, and I think it relates to the question under discussion, that something further might be done than was suggested by Mr. Mark Robinson—something on the lines of our friends in America, but not quite so much as has been done with the Heilman locomotive in France—namely, to have moving generating stations in which the current is produced upon a vehicle and charged direct into the third rail, such a vehicle to be provided with a motor of only sufficient power to move itself at a moderate pace. Suitable engines of a high-speed type and generators would be provided, and the whole would form an electric locomotive not necessarily to be used for tractive purposes excepting in the way I have indicated. The advantages, from a railway traffic manager's point of view, would be very great, because such moving electrical plants could be distributed where the greatest power is required, and thus get rid of one disadvantage at least, viz., that of a fall of voltage due to generators being so far away from their work. Mr. Hoy.

Mr. J. S. RAWORTH: We have the pleasure of seeing Mr. Langdon once more amongst us reading a paper. I want to say one word about Mr. Langdon's paper, and to call attention to the thanks that we owe him for the industry and perseverance with which he puts these papers before us. Mr. Raworth.

Mr. Raworth. I have followed Mr. Langdon's figures pretty closely, and mine do not come out quite the same as his, but it will be better if criticisms come from our side rather than the other. The President has very properly called on the locomotive superintendents present to come forward, and I have been wanting the man to come forward who would say that Mr. Langdon is all wrong, and that his suggestions could not be carried out, for we would at once refute his arguments. I have looked over Mr. Langdon's figures, and on Table II. he says that the maximum number of trains per hour passing through Luton is sixteen, but in the column on the right-hand side you will find there are nineteen. Then you find in Table III. that ten trains more go through Harpenden per day than through Luton, but they are, curiously enough, all light engines. I do not know whether Mr. Langdon will be able to give us an explanation of the fact. When we come to a calculation of the power required to drag trains from London to Bedford, I am sorry to say I have not been able to follow Mr. Langdon quite clearly through his calculations. He has adopted a nomenclature of his own, and though I have succeeded in mastering it to some extent, I thought it better to recalculate the whole from the beginning. On Mr. Langdon's figures I took out the total foot-pounds that would be required to drag the 287 trains over the fifty miles in twenty-four hours, and then I have divided that out and brought out the total result at 7,380 kilowatts, with an average of twelve trains per hour. Then providing for a possible maximum of sixteen trains per hour, not nineteen, and allowing 600 kilowatts for acceleration, with four stops on the average in the distance, I find that the total kilowatts required is 9,850, which shows that Mr. Langdon, after all, is not such a bad guesser in fixing the power of the station at 10,000 kilowatts. I do not think we shall grumble at Mr. Langdon because he has allowed a fair margin that will cover any method of calculating the results, because it must be clearly understood that results cannot be found out exactly ; they must, in every case, be subject to a certain amount of guess work. For nstance, Mr. Langdon's formula of the tractive power per ton is $3 + \frac{V^2}{250}$ miles per hour. Although I am not now a railway man, I was once, and I think there must be some little error in the formula of Mr. Langdon's, because you will observe that if we only went one mile an hour we should have no more force to push the train than 3 lbs. per ton.

Mr. Langdon. Mr. Langdon : I should have mentioned that the formula is not applicable under five miles an hour.

Mr. Raworth. Mr. Raworth : I give in at once. There are one or two points in Mr. Langdon's calculations in which, I think, he has in his usual kind manner given the benefit to the steam locomotive. In the first place he has taken the expenses for the last twenty-four years of the steam locomotive, and he has taken expenses for the present year with the electric means of propulsion. You will readily imagine that this is very much in favour of the steam locomotive. I think a few years ago the locomotive took from 29 to 30 lbs. of coal per ton-mile; now it takes from 50 to 51 lbs. A few years ago wages were very much lower than they

are now. We have to pay on the higher scale whereas the calculation is made upon the lower scale. Mr. Raworth.

Mr. H. A. Mavor: We are all very much indebted to Mr. Langdon for bringing this paper before the Institution. What I wish to say is that, if agreeable to the Council of the Institution, we of the Glasgow section would like the discussion kept over until after the [illegible] of December, so that we may arrange to have a full discussion of the paper to be embodied in the Transactions of the Institution. We have in the north a large number of very highly skilled locomotive engineers who are able to discuss the question, and we hope that our contribution to the discussion will be worth recording. Under these circumstances I will not take up more time in discussing the question now. Mr. Mavor.

Colonel R. E. Crompton: I am unfortunately not prepared to do justice to this most interesting paper, as I intended to speak during the adjourned discussion, but an engagement prevents me from doing so. I must, therefore, confine myself to a few words this evening. Colonel Crompton.

This is a case where the importance of the matter calls for the very best opinions of the electrical world, and I hope that these will be given very freely during the discussion.

I am particularly appreciative of this paper, as I have designed an electrical railway 160 miles long worked from three stations approximately 40 miles apart. This is a Trans-Himalayan line, and to be worked by water-power. In the course of studying the project, it was necessary to prepare data very similar to those given in Mr. Langdon's Table IV. If I had had that table before me it would have greatly facilitated my labours. It happens that I did prepare my time-table of trains on lines very similar to those followed by Mr. Langdon. One of the first points which I have noticed is the extraordinary discrepancy between the horse-power taken by the trains, as calculated for in column 4, and the actual horse-power of the locomotives when working to their maximum efficiency; in fact this descrepancy at once excited the interest of us electrical engineers, who have so long been accustomed to talk about the load-factor of our steam engines, and the effect that it has on their economical efficiency. It is evident that the comparatively small load-factor of a steam locomotive working an ordinary railway is most probably one of the causes why the locomotive which, taken as a whole, *i.e.*, boiler and engine together, ought to be an economical machine, is not in practice found to be really economical. This small load-factor also explains why the introduction of compound steam locomotives has been so delayed, and its economical advantages are even now disputed by some locomotive engineers.

Mr. Langdon shows, and it is evidently the case, that the substitution of fixed stations, from each of which the power required for several trains would be supplied, must be a means of improving the load-factor of the generating plant which supplies that section of the line.

Turning to another point, that of the wages, which Mr. Langdon shows is a very important item in the cost of working steam locomotives. I do not think he is quite fair to the electric locomotive when he debits it with the same wages as the steam locomotive. No doubt he is wise in thinking that at first there would be no great saving on

Colonel Crompton.

this head, and that if the directors of an electric railway attempted to economise in this direction the public would be alarmed, and would imagine that unskilled men were employed. But I wish to point out that the drivers of steam locomotives are exceptional men and have to be paid an exceptionally high rate of wages, because they are not only men of great nerve and judgment, as regards the mere driving of their trains, observing signals, and other matters which affect the safety of the travelling public, but they have to combine with this a highly skilled training in the management of the locomotive as a power-producing machine. I refer, of course, to the best methods of working the steam, times of firing the boiler, and other things necessary to get the highest duty out of the boiler and steam engine. Now the driver of the electric locomotive need only be skilled in the first part of these duties, and this fact alone ought to reduce the rate of wages paid.

I will touch on another point, and that is the question of repairs of the locomotive machinery. I think these will be far lighter in the case of the electric than can ever be the case with a steam locomotive. For instance, the points at which wear takes place to the greatest extent, and which cost most to repair and keep in order, are the sliding surfaces exposed to the weather, dust, mud, rain, etc.; these sliding surfaces, such as those of the piston-rods, guide-bars and links of the steam locomotive, are absent in the electric locomotive. The friction surfaces of the electric locomotives are confined to journals, and these can be far more easily protected from the weather than the sliding surfaces. Again, the locomotive boiler, excellent though it be, costs far more money to maintain than the stationary boiler of the electric system. Again, I think that the substitution of driving by a large number of axles on the train will greatly reduce the weight on these driving-wheels necessary to obtain adhesion, and this will very greatly reduce the cost of renewal of the permanent way.

While on this matter, I must point out that Mr. Langdon has not made a point for the electric system which he might have made. I think there are signs that in the future our steam locomotives will be overloaded, *i.e.*, they cannot be made big enough and powerful enough to haul the traffic at the required speed; they are limited by the gauge, position of platforms, bridges, etc. Already in some cases the diameter of the driving-wheels must be reduced in order to get in overhanging boilers, so that some modern locomotives are becoming like camels (at least, I think that this is what locomotive engineers are calling them).

Again, they are limited in length by the existing turn-tables. All these are signs that the steam locomotive power has reached its limit, and it is only by distributing our motive power over a considerable proportion of the train itself that we can increase the driving power and hence the speed. I think this is a strong argument in favour of the introduction of the electric system.

The Chairman.

The CHAIRMAN announced that the scrutineers reported the following candidates to have been duly elected :—

Members:

Herbert Broadbent. | John Somerville Highfield.

Associate Members:

James Anderson. | Arthur Woodroffe Manton.
Marcus Nash.

Foreign Member:

Otto Peder Krogh.

Associates:

Harry Watkins Kimber. | Arthur Henry Pook.
Thomas Mills. | David Shanks.

Students:

William Beale Cole. | John Marshall.
Ernest Ferdinand Motta.

The Three Hundred and Fifty-Third Ordinary General Meeting of the Institution was held at the Institution of Mechanical Engineers, Storey's Gate, Westminster, on Thursday evening, December 6th, 1900, Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on November 29th, 1900, were read and confirmed.

Donations to the Building Fund were announced as having been received since the last meeting from Mr. A. T. Snell and Mr. T. Mills, to whom the thanks of the meeting were duly accorded.

Messrs. H. L. Leach and W. H. Merriman were appointed scrutineers of the ballot for new members.

RESUMPTION OF DISCUSSION ON PAPER BY MR. W. LANGDON, VICE-PRESIDENT, ON THE "SUPERSESSION OF THE STEAM BY THE ELECTRIC LOCOMOTIVE."

Mr. Hammond.

Mr. ROBERT HAMMOND: I think we must all be struck in reading this most valuable paper of Mr. Langdon's at the immense new field which he is opening out for the young as well as the old electrical engineers. One cannot help, when viewing this vast field, being reminded of the great achievements that our modest force of electricity has made in the past. All must have enjoyed the speech made by Sir William Preece in the discussion on Mr. Gavey's paper, when he pointed out that more than twenty-five years ago the Institution marvelled over the great discoveries that were being made in the application of electricity to the telegraph, and how every new invention succeeded, by the utilisation of the wires then in existence, in very greatly enlarging the power of the electric telegraph. I was not a member of this Institution in those days, but I can quite accept Sir William Preece's account. It is, however, within my memory, and within that of many in the room, that our members generally were astonished to know that electricity was going to compete for lighting with the great gas industry. When the bold conception was started that we should be able to distribute electricity from a centre and use it for lighting our houses, those of us who were pioneering that movement were much scoffed at. Well, the *Journal of Gas Lighting* notwithstanding, we have lived through that period, until we are beginning to consider that it is a matter of astonishment that there is still a gas light in existence. Then some few said, "If we can distribute energy for doing away with gas lighting, why not distribute it for the utilisation of our factories?" and last year we have seen the first great step that has been made in the passing of those

important power Bills. [illegible] eminent financiers in different parts of the country [illegible] that is to finance and to distribute power from a central [illegible] to great factories; and if all the arrangements of these various groups of financiers are realised, electricity will [illegible] industry in the land. Then, lastly, we have Mr. Langdon's [illegible] painstaking and valuable paper [illegible] another field in the electrical engineer's [illegible] the supersession of the steam locomotive. There have been [illegible] put forth at meetings of the Institution [illegible] constant railway traveller there hardly seems a happier suggestion than that we should do away with the [illegible] and get so uncomfortable and [illegible] with dust and dirt [illegible] the same way as we formerly were content to be illuminated at night with a light that was competing with [illegible] which is necessary to life. Mr. Langdon comes before us with a paper that [illegible] generalities. We are all indebted to Mr. Langdon for the paper, because he not only shows that such a thing is in his opinion possible, but he also shows from beginning to end how such a thing can be carried out. He does not hesitate to suggest certain methods of generation and distribution of electrical energy. He sets out the capital outlay which his scheme will cost. He deals with an actual fifty miles of line. He goes into the question of how he would place his sub-stations, his primary and his secondary voltage, and he works out the capital cost. That seems to me a very practical way of dealing with a paper of this kind. Finally he compares those costs with the costs that are, and have been for so many years, duly recorded by the steam railways, and he shows that on the average cost of a large number of years he is prepared to show a saving of over a quarter of a million. In taking the average cost over a great many years he has taken a figure which is really unfavourable to his argument, because it is well known among railway men that costs have tended to increase. Mr. Raworth, in a particularly happy speech at our last meeting, deprecated a criticism of the details of Mr. Langdon's scheme, and begged us to confine ourselves to the financial aspect. Of course it is manifest to us that, as far as the actual directors or railway shareholders are concerned, they care little about the methods, and they will adopt this new plan or not accordingly as it will increase or make more certain their dividends. But I cannot follow Mr. Raworth in his suggestion that the proper way of dealing with Mr. Langdon's paper is to deal with it only as a question of principle. He has laid before us a certain scheme. Mr. Mark Robinson, for instance, in opening the debate, ventured to point out that in his opinion, instead of have one central station, economy might be obtained by having some five central stations; and I think it would be the best compliment we could pay to Mr. Langdon, and would lead to the most practical outcome of this debate, if we discuss the details of this paper somewhat instead of confining ourselves to the general principle. The two main points of Mr. Langdon's paper which we, as engineers, should consider may easily be summed up. The first is, Is the plan of running full-gauge railways [illegible] electricity feasible? and secondly, Are the estimates that he lays [illegible]

Mr. Hammond. us in connection with this scheme sound or not? First, with regard to the feasibility. I think we may unhesitatingly let it go forth as the opinion of this Institution that that plan is feasible. I think we have sufficient data before us to say unhesitatingly that full-gauge railways can be run by electricity, and we are encouraged to say that because it is actually being done at the present moment. For the past eighteen months a line has been running, as many members know, from Burgdorf to Thun—a full-gauge line which was formerly worked by steam locomotives, and is now being worked by electric locomotives. That line has a length of about 25 miles. Comparing it with the plan laid before us by Mr. Langdon, I may recall the fact that the primary voltage there is 4,000 volts, and the secondary voltage is 750. It is 4,000 volts at the generating station, raised up to 16,000, and then it is lowered down to 750 on the 3-phase system. In addition to that line, there is another very much larger line. A line is being put down by the Shuckert Company and Ganz Company of Buda-Pesth for about 70 miles, and it will, when it reaches Milan, be 110 miles long. On the Burgdorf-Thun line the sub-stations are placed at distances of two miles apart, whereas on the Northern Italian line they are at distances of 10 miles. There the extraordinary plan is being introduced of running with a primary voltage of 20,000, and reducing it only to 3,000 on the motors on the car. When I was in Buda-Pesth a short time ago, I had the pleasure, in company with Mr. Blathy, of running over the trial portion of the line. They have laid down an actual track and are running cars upon that line, where the primary voltage is 20,000 and the secondary voltage in the car is 3,000.

With regard to the figures, I have gone very carefully through them, and I consider the estimates of working costs are sound. I consider that the points thrown down by Mr. Robinson are worthy of consideration. We must remember that the two lines I have referred to go from a central station, because at the central station there is water. Our problem in England differs from the problem in almost every part of the world in that we have coal apparently at 7s. 11½d. per ton, which can be easily taken to the point where it can be best used.

The President. The PRESIDENT: Before calling on Professor Forbes, I will ask the Secretary to read a letter received from Mr. Hoy, the locomotive superintendent of the Lancashire and Yorkshire Railway, who spoke last week.

Mr. Hoy. Mr. H. A. HOY (*communicated*): I should have liked an opportunity to point out to Mr. Langdon a misconception on his part which occurs on page 142, in which he says: "The main gain, however, is to be found in the economy of a stationary, as against an itinerant generator, as well as in the fact that much coal is consumed by goods and mineral trains when shunted, and by all trains when standing at stations, the whole of which would be saved if worked by electricity."

I beg to say that this is not so; in fact, very much the reverse. Mr. Langdon's stationary engine would be continually revolving, and coal will be consumed to produce these revolutions, although it may not be doing any legitim[illegible] On the other hand, with a locomotive the moment it [illegible]d the wheels cease to revolve,

and the consumption of fuel in the firebox is a negligible quantity, and the conditions of economy are more favourable than those which take place in the domestic fire-grate. An engine driver who knows his business, when he is put into a siding or when he stops for any length of time, closes his damper and opens the firehole door, thus preventing all draught from passing through the grate, a course of treatment which, if continued, would result in the fire going out altogether. Mr. Hoy.

The amount of power derived from the coal consumed in a locomotive firebox of limited area is so great that the fire is most sensitive to the fluctuations of load, hence the economy; whereas in the case of fuel being burnt in large stationary boilers, the effect upon the fire when the load on the stationary engine alters from maximum to minimum at intervals is not noticeable. Again, for the same reason, due to this elasticity of generative power, a large amount of coal is consumed in getting up steam and a proportionately large amount of fuel wasted if the load unexpectedly goes off for a considerable period.

Professor G. FORBES: I have very little to say, and that may make it appear that I seem to be critical. I do not want my remarks to be considered in that light. Practically my criticism would amount almost altogether to this: That this is hardly a paper to be brought before this Institution; it is of too general a character. It evidently has not been worked out in its full details. The various methods by which the work could be done have not been considered, nor have the details of cost been at all accurately gone into. It does not matter in the slightest that the result obtained with a stationary engine is cheaper than with a locomotive, which is practically the result arrived at in this paper. All I wish to say is that if Mr. Langdon had read this paper before the Society of Arts, or before some popular audience, or had published it in some more popular form, it would have deserved approval in every way, because it draws attention to a subject which has been interesting engineers for a large number of years, and which, although not novel to those who happen to be engaged in those subjects, is novel doubtless to a large number of people generally interested in the matter. I have said that the subject is an old one which is well known to all those who have been engaged in discussing the question of the electric working of railways. If, in elucidating this point, I draw attention to the work that I have done myself on the subject, it is not in the least with the idea that I stand alone. The particular point that has been raised to-night in Mr. Langdon's paper has been common conversation among us all for the last quarter of a century. You will find that the only facts in the paper before us that have not been available for everybody who has been discussing the question for the last quarter of a century are the number of trains that are passing two stations on the Midland Railway. The whole of the rest of the facts are perfectly accessible to everybody who has been discussing the question. And here let me point out, while alluding to those two stations, that Mr. Langdon has chosen the most favourable instance that was within his cognisance to illustrate the electrical side of the question. A most important point has been drawn attention to in the letter which has just been read to you, namely, that no such Professor Forbes.

Professor Forbes.

system for supplying electric power from central stations can, in the nature of it, pay its dividend on the capital invested unless you have a continuous service of trains. If you are going to have only a train or two an hour, or a train every two hours, the stationary engine loses a great deal of its benefit during the idle moments. I will just give you some idea of how people have been working on this subject for many years. I do not in the least wish to push myself forward as having done anything in the matter, but I have taken a note of the things which I happen to have been at work on in connection with the subject. I first drew attention to the desirability of introducing electric traction on lines of railway in a lecture at Glasgow in 1879. In 1881 I communicated to the *Times* an account of the same thing, while in the following year, before the Society of Arts, I drew attention to the same subject. Continually from that time onwards myself and others who are interested in these matters have been discussing this problem, and they long ago arrived at the conclusion which is contained in this paper, namely, that a stationary engine uses less coal than a locomotive engine. After that, it has happened that in the ordinary course of one's profession I have been obliged to investigate very closely the different methods by which such a service can be given. At the time when the City and South London Railway was started, Mr. Greathead invited me to report on the different methods in which that railway might be run. You remember it was originally intended for a cable railway, and that was the first practical case which I had to deal with in which figures of cost and working expenses were wanted. Subsequently I did in a rough sort of way do something in the United States dealing with the same question. During the years 1897–98 I was employed by the Egyptian Government in surveying and reporting upon the capabilities of the Nile cataracts. Lord Kitchener, then Sir Herbert Kitchener, the Sirdar, consulted me about his desert railway from Wady Halfa to Abu Hamed. I pointed out to him that after the first 100 or 150 miles had been laid, out of every four trains which he would be sending from Wady Halfa to railhead, three trains would be carrying nothing but water and coal, and I suggested to him the desirability of introducing stationary engines at Wady Halfa, and laying down wires to work electric locomotives to carry materials for construction. He thoroughly appreciated the idea, and would undoubtedly have carried it out, but time was pressing, and it was impossible to get materials for this work quickly enough. At the same time, while I was engaged upon this service, Sir William Garstin, Assistant-Secretary of Public Works, asked me to pay attention to the feasibility of working the Nile railways by electricity. In that case I had to go extremely fully into the details of capital expenditure and working expenses. I have had to work on the conditions of other railways in different parts of the world in the same way. In 1897 the editor of the *Engineering Magazine*, whom I had not communicated with, happening to know that I had done something in this way, asked me to write an article for that magazine, and I wrote a very popular general account of the work.

"The conclusions derived from study extending over many years are as follows:—

Professor Forbes.

"(1) In cases where water power is always available within a few hundred miles of a trunk line of railway, it is probable that economy would be served by introducing electric traction.

"(2) In the case of an independent system of railway to be constructed in a new country utterly unaffected by the traffic from steam railroads, power can be applied to every axle of the train; wherefore it will be economical in such a case, in construction and in operation, to use electric propulsion in preference to steam.

"(3) For desert railways, where water cannot be obtained, electric traction is eminently suitable.

"(3) In underground railways, such as the Baltimore Tunnel and the London underground system, where economy is not so important as convenience and comfort, electricity must be employed; and, where such railways are to be constructed, economy makes electricity advisable.

"(5) In cases of suburban traffic electricity would help to overcome the competition with street railways by supplying the public with separate and independent cars running at very frequent intervals on a well-maintained track."

That is really the same result that has been arrived at by Mr. Langdon and by many others in looking into this question. Mr. Hammond has said that one ought to look into the question of the details of the scheme which Mr. Langdon has put before us. I do not think Mr. Langdon would claim to have gone really into all the different ways in which this could be done, otherwise I would criticise the paper very severely. At any rate, I would put forward very opposite opinions to those which are contained on page 133:—

"Midway in the fifty miles of railway is the central station, containing four 2,500 kilowatt, three-phase, or other characteristic, 10,000 volt generators. At this pressure current is distributed to sub-stations, each serving ten miles of railway, where the potential is converted to 600, from whence it is carried to the contact rail. Or the centre ten-mile section may be provided for by direct-current generators served from the same steam plant."

I can only say there is not a single item there which seems to me to be the right thing for the purpose. I know it is not fair to state things in that general way, but if I were to go into the whole methods by which I consider that such a railway ought to be worked, it would take up far too much time, and would be the result of much more laborious calculation than the paper itself has been. I wish also to say, since Mr. Hammond has said we ought to go into the details a little, that I have looked into some of the figures to a slight extent. I have looked into the £70,000 that he is going to spend on his go and return rail. All that is based upon 600 volts pressure, upon a distance serving five miles from each sub-station, and the general result I get at is that, if it is copper, 6·6 square inches would be required to give 10 per cent. drop at the midway point, which I understand is what he wants. In that case the total expense on the low-pressure conductors, if I have understood the paper properly, would be £330,000 instead of £70,000; but I may have misunderstood some point in the paper, and therefore I will

not say more. I am afraid that I have seemed more critical than I really wanted to be. If this paper had been simply a general paper to the public at large I have already said it would be a most admirable paper, most beautifully put and expressed, but as there is nothing in the paper that has not been better done before, I hardly think it has gone into sufficient detail, or is sufficiently technical for the Institution of Electrical Engineers.

Mr. F. HUDDLESTON: I hardly agree with the last speaker that it is unnecessary to deal with details in these matters. I think the chief points that one can deal with in a paper of this magnitude are the details of various savings which Mr. Langdon has put before us. I propose to confine myself generally to the coal consumption that he has spoken of. First of all last week Mr. Raworth touched upon what I think was a serious mistake in Mr. Langdon's figures. He tackled Mr. Langdon's coal output, as shown in the last column of Table No. IV., and said that he had made out practically double that amount. He did not follow this to its conclusion, but in my opinion he was right. I think Mr. Langdon has entirely under-estimated the work on heavy railways such as you have to deal with in steam-locomotives. If you look at Table IV. you will find the speed given in the fourth column, the load in the sixth, and the total tractive effort in the seventh. He has calculated out his Tractive Effort on a formula which, as Mr. Raworth pointed out, is fairly correct for high speeds, but is not correct for low speeds. Moreover, he is assuming a perfectly straight road, and a level one. These things do not exist in practice. You have in reality uneven roads, and you have heavy ground to go over. If you divide the tractive effort by the load you will find that Mr. Langdon gives this startling result. He says he is going to deal with a mineral train and a goods train with a resistance of only [illegible] lbs. That is an impossibility on railways in England. The ordinary goods train pulled along has a resistance of at least 10 lbs. on a straight road, but on an ordinary irregular road the resistance is increased enormously. The train has to be pulled uphill; on going downhill the brakes must be put on more or less, and in addition there is frequent starting and stopping. Hence the power given out is much larger than the author states — 18[illegible] H.P. to pull 500 tons. I propose to take my facts from Mr. Johnson's paper, which, as Mr. Langdon remarks, is most valuable for an inquiry of this sort. I have taken chiefly the year 1892. The figures are given in Mr. Johnson's paper, and it will be found that in that year Mr. Johnson carried out for the Midland Railway Company a set of experiments to find out what was the coal consumption on passenger trains, goods trains, and mineral trains. The figures are all set out in Mr. Johnson's paper, and he gives the results in pence per train mile. He also gives his average price of coal, and if the average cost of coal per train mile be divided by the price of a pound of coal, the average consumption for the three classes of trains is found. Passenger trains took about 36 lbs. per train-mile, goods trains 53 lbs., and mineral trains 62 lbs. That agrees with all the other results that have been obtained on railways in England—there are dozens of cases. There was a paper read before the Institution of Civil Engineers in

Mr. Rudleston

1895 by Messrs. Adams and Pettigrew, in which they gave a set of elaborate experiments on the South-Western Railway. They were trying an express train, and took diagrams during the whole of the run. They found there with a train of about 250 tons weight, that it took about 30 lbs. of coal. It was a fairly easy route, and I expect Welsh coal was used—in fact they say so. Mr. Smith read a paper before the Institution of Mechanical Engineers about two years ago on a similar set of tests on a North-Eastern express of about the same weight—275 tons. There his coal consumption was about 40 lbs., a little higher than in the other case. That was ordinary, good North-Country coal. Then Mr. Webb on the North-Western took a party of engineers up to Crewe with one of his compound engines. Everything was tested pretty closely. The train weighed 420 tons, and the coal consumption was about 44 lbs. per train-mile. All these figures average about the same per ton-mile. In one case it is 0·14, in another 0·105, and on the South-Western about 0·11 lb. The horse-power obtained was, on the South-Western express very nearly 600 H.P., and on the North-Eastern from 550 to 800, which averages over 700. Comparing those figures with the first figure given by Mr. Langdon in his table of 477 they are very different, and I should say a train of that weight will require an average horse-power of about 600 to pull it along. Ordinary passenger trains and empty coaches require very little less power, because they are always starting and stopping, and there is much to be done in acceleration. For a train of that size you should not, at a moderate estimate, have less than 350 H.P. In the next case, Mr. Langdon puts down 295 H.P. for a train of 400 tons at a speed of 35 miles an hour. It is more likely to be over 400 H.P. A mineral train, according to Mr. Johnson's figures, burning roughly the same amount of coal, would come out at about 400 H.P. Any locomotive superintendent would say that a mineral train of that weight would not take less than 400 H.P., and he could probably prove it by diagrams if he chose. Taking these results, we get 6,100 H.P., instead of the total horse-power which Mr. Langdon has got in his last column but one. That agrees fairly well with what Mr. Raworth has said. The net result is, that the power-houses, instead of giving about 5,000 kilowatts, would have to give nearly 9,000.

Generally speaking, I should say that Mr. Langdon has under-estimated the whole of his power, and that he would require an output of nearly double what he has mentioned. Putting it at 9,000 for the moment, I am afraid the coal consumption of 30 lbs. per train-mile will go up considerably—it goes up, in fact, on my figures to 57. Mr. Langdon, again, calculated on the consumption of 3 lbs. per kilowatt at his power-house. We have not got that yet even in the steadiest load of any power-station in England; but, on a railway, the load is not a steady load. Mr. Langdon has an average of about fourteen trains passing through. The power they take varies enormously from time to time, and a large compound engine, however good it may be, cannot work economically with a load that varies, we will say, from 60 up to 140 per cent. I have known a railway vary from 50 to 150 per cent. on its average. You cannot, under those circumstances, expect t

Mr. Hudleston. generate your power for as low a coal consumption as 3 lbs. I am sure, at least under our present knowledge of steam and everything else, that something like 4 lbs., or 4½ lbs. per kilowatt, would be used. That brings up the consumption to 85 lbs. per train-mile, instead of 50, as in the case of the Midland at present. There are several electric railways in England, and two of them I know well. Both of them have compound condensing engines in the power-houses; both of them have more trains per hour than Mr. Langdon has, and it is found that their coal consumption does not compare favourably with that of the Metropolitan District Railway. The coal consumption of these railways has been stated at various times from 0·4 up to 0·5 lb. per ton-mile. I think Mr. Cunningham last week gave ½ lb. as the rate for the Central London, and about 0·4 lb. for the Overhead Railway; but Mr. Cunningham was perhaps a little beyond the mark in his figure. Yet taking 0·4 lb. for the work done on these railways, you cannot at once compare with the kind of railway that Mr. Langdon has spoken of, because they are for short distances, with frequent starting and stopping, and have to do an enormous amount of work in accelerating. But the Underground has much the same conditions of traffic. It is practically level from station to station, and has no switchbacking. The Liverpool Overhead Railway is almost the same: it has one switchback length in its line. On the Central London, which runs at a much higher speed than the other two, switchbacking does something like thirty per cent. of the whole power required to pull a train from one end to another at that speed—that is, the switchbacking does most of the work of accelerating. Therefore I say one may take these two railways, the Central London and the District, as a very fair comparison of what can be fairly well done now by electrical engineers. The District Railway does its work at 32 lbs. per train-mile; the Metropolitan does it at 36. The weight of the train is about 135 or 140 tons, very nearly the same as the Central London, and that works out at rather less than ¼ lb. per ton-mile. The District Railway uses the best coal it can get, simply because of the fumes that are given out. I do not think you can fairly claim that the difference between 0·4 and 0·25 lb. is entirely due to the South Wales coal, though I would ascribe a certain amount of it to that. I think that the electric railways I am dealing with, if they were burning good South Wales coal, would probably get down to very nearly 0·3, but they would have a difficulty in getting past 0·25 lb. This agrees pretty well with what I put down for the figures on this ideal railway. I think myself that Mr. Langdon has under-estimated it considerably. With our present knowledge of electricity and steam, I do not see how one can expect to get an economy of that sort. I believe in a very short time we may get down to the same coal efficiency, but I do not think it will be surpassed, and therefore I think that this particular economy must be wiped off the sheet. After that, I admit all the other things are an advantage—the electrically driven lines are cleaner, and sweeter, and the hauling is more regular. The dirt is got rid of, and there is not the same wear and tear. As regards repairs and so forth, we have had no experience yet to judge, but Mr. Langdon, fairly enough, assumed that they will be about the same. I think the coal economy is not

proved in the least, and that many of the other economies will hardly stand close inquiry. Each particular item of Mr. Langdon's paper—I do not agree at all with the last speaker—is worthy of criticism. Mr Hudleston.

Mr. A. A. C. SWINTON: I would like, at the outset, to say I do not agree with Professor Forbes that this is a paper not suited to this Institution. I think that it is not only eminently suited, but that it is one of the most interesting and suggestive papers we have had for a very long time. About a year ago I had a conversation in London with a very eminent authority upon matters of this kind—Mr. George Westinghouse; and, as far as I could gather, he did not appear to contemplate the possibility of the adoption of electricity for driving trains upon main lines. He thought that very shortly suburban traffic would be almost entirely done electrically, but he did not seem even to have contemplated the use of electricity for driving trains on main lines, such as is dealt with in this paper by Mr. Langdon. Only about a year ago I had occasion to look into this question in connection with a contemplated railway, and made some estimates as to what the scheme would cost. Curiously enough the line was to be exactly the same length as that portion of the Midland Railway which Mr. Langdon has taken as his basis in the paper before us—it was just over fifty miles. After a careful computation, I came to the conclusion that the total cost of the equipment would be little more than Mr. Langdon has stated—namely, just over £523,000; but there the resemblance, I am sorry to say, ceases. I must confess that I cannot understand Mr. Langdon's figures in regard to the subject to which Professor Forbes has already drawn attention. I cannot understand how he is to make the necessary arrangement for cables and contact-rail for £140,000, and I think it would be very interesting if Mr. Langdon would give us further details as to what is included in those two items. In my case, the power was much less, as there were fewer trains running at one time; and although my total came to very much the same as Mr. Langdon's, a much smaller proportion was represented by generating plant, and a very much larger proportion by plant for distribution, by which I mean cables and "trolley wires." Mr Swinton.

There is one way in which, I think, some economy might be derived of which Mr. Langdon has not taken advantage. Mr. Hammond mentioned a railway where they are going to employ 3,000 volts upon the motors, and I do not know anything, except perhaps the rules of the Board of Trade, to prevent the use of a considerably higher voltage than 600 upon the motors; and if, in addition to using higher voltages on the motors, we can also successfully use three-phase motors and save the rotary transformers, there will not only be a considerable saving in capital, but there will also be an increase in efficiency, and, more important still, there will be no necessity for any assistants at the sub-stations. Rotary transformers necessitate the employment of assistants, but static transformers can be shut up and left to themselves, and that will make a considerable difference in the cost of running the concern. The evening this paper was read, I happened to meet Mr. Charles Brown, and thinking it a favourable opportunity to ask him about the Burgdorf-Thun Railway, I inquired whether, if he had to equip

Mr. Swinton. electrically a main line in this country, he would use the same system; and he replied that, unless there were any very exceptional circumstances which did not occur to him at the moment, he should certainly use three-phase motors, and not continuous-current motors and rotary transformers.

The question raised by Mr. Robinson as to whether there is economy in large stations is a very important one. Some of us spent many weeks last year in trying to persuade a Committee of the House of Commons—and we did persuade them—that there was economy in large stations. If there is no such economy, I can see no advantage in Electrical Power schemes. Some people seem to think that the whole matter is merely a question of how many pounds of coal are required to produce a horse-power in an engine; but the coal expenditure is but one item out of many. There is the question of capital cost. Does Mr. Robinson suggest that a station for 10,000 kilowatts costs ten times as much as one for 1,000 kilowatts? [Mr. MARK ROBINSON: It costs twice as much as one for 5,000 kilowatts.] I cannot concede even that. In dealing with 10,000 kilowatts you can afford to do all manner of things that you cannot with 1,000, and which you cannot do so well with 5,000 kilowatts. At the end of three weeks the condition of the mind of the Committee to which I have referred was that at about 10,000 kilowatts the *rate of increase in* economy began to slack off. But that is not my opinion. I should not be inclined to put it below 50,000 or 100,000; indeed I am not sure you can put it anywhere. The bigger the plant is, the easier it is to arrange all manner of different economising devices, such as superheating, coal-conveying apparatus and the like.

I would also like to ask how, with stations all along the line (as Mr. Hoy suggests, with stations about every mile), a constant load is to be obtained. Each station will be working for about five minutes, and then it will have to shut down. A constant load can only be ensured by having one station to work a great length of line, and it is only by securing a constant load that proper economy can be obtained.

Mr. Walton. Mr. A. H. WALTON: I cannot agree with my friend Mr. Hammond, who has previously spoken, that everything is just as it should be in this paper. Mr. Langdon has certainly put it before us in a very broad manner, but I think he has got wrong over his tractive effort. His figures for mechanical horse-power are, as Mr. Hudlestone said, far too low. I will give you an instance. Take his express train having a H.P. of 477. That works out as low as 26 watt-hours per train-mile. I do not think it has ever been done yet at that figure. The best we know of up to the present time is a little under 40 watt-hours per train-mile. Molesworth's formula, defining the resistance as lbs. per ton $= \frac{V^2}{171} + 8$, level, gives a tractive effort for that express train of 22·6 pounds per ton, which is somewhere near Mr. Hudlestone's figure. Mr. Hudlestone mentioned the figure of 827 against Mr. Langdon's 477. If we go still further, and work out the watt-hours per train mile, Mr. Langdon's come out at 26, whereas Molesworth's come out at 44·8, which, I think, is near the mark. I think you will agree with me that Mr. Langdon has altogether under-

estimated the power required for the tractive effort. It is true his comparisons are with actual data at his disposal in Mr. Johnson's paper, that is to say a mechanical H.P. costs so much a train-mile with a given weight, but he has had to estimate the electrical side of the question, and his estimate is altogether too low; indeed, I think with Mr. Hudlestone, it ought to be wiped off the sheet. With regard to Mr. Langdon's statement on the question of coal, he has put down 3 pounds per kilowatt-hour; but I agree with Mr. Huddleston that from 4·5 pounds to 5 pounds would be considered fairly good in practical working. I think we shall be very well satisfied if we get it in the many projects that are before us and, at the same time, reduce some of the stations that are already running to that figure. I do not think 3 pounds per killowatt-hour has ever been accomplished. Mr. Cunningham told us that on the Central London Railway the watt-hours per train-mile were 70, irrespective of the lifts and lighting. Ever since we completed that line under Mr. Hudleston's supervision we have been watching it very carefully, and we find that the watt-hours per train-mile are 64·9 actually generated for everything, and we find an average at the third rail during the last two months of 50 watt-hours without the lighting of the train. That average has been checked by one-second readings during three or four runs with the motor from the Bank to Shepherd's Bush and back, and the figures actually came out at 49·5 after checking instruments, so that I do not think we are far wrong. Mr. Cunningham also told us that the coal was 0·5 lb. per ton-mile. Mr. Hudleston, I think, has already said something in reference to this. In watching this point we found that the actual coal per kilowatt-hour was 0·44 pounds, which works out at 6·6 pounds per kilowatt-hour. That, of course, is high. Here I would venture to suggest that we seem to be talking in the dark on this question of coal. We heard Mr. Hudleston speak of what can be accomplished with Welsh coal, and he said that we could not get down to a particular point if cheaper coal were burnt; but in this instance it is entirely due to the very cheap fuel which is being used. I venture to suggest that if we could standardise this question and speak of the cost of the coal, and not of the weight of the coal, it would be much better. We are all burning different kinds of coal, and no one gives the calorific value. One is burning Welsh coal, and another is burning practically dirt. Mr. Walton.

There is just one other question I should like to touch on. On page 135 the author suggests 2½ per cent. leakage on his line, which I consider is very excessive. Taking the five sections given at Figure 2, that amounts to 35 amperes per section, or on the total line 170 amperes, and taking the resultant of the E.M.F. over the current which is passing through, we get the most extraordinarily low insulation result of 3·5 ohms. I ask, Is any line going to work on that? I am afraid Mr. Langdon has rather overdone it in the case of leakage; and that he has been too liberal on the electrical side.

Mr. A. J. LAWSON: We have all to thank Mr. Langdon for his paper. While I agree to some extent with Mr. Hudlestone and others that the author has taken too low a figure for coal consumption, I think he has, on the other hand, done some injustice to his own cause by assuming Mr. Lawson

Mr. Lawson. much too low an efficiency in his motors and in his method of transmission throughout. For instance, he has taken the motors with 85 per cent. efficiency; the ohmic loss in transmission at 10 per cent.; leakage $2\frac{1}{2}$ per cent.; loss in rotary converters 10 per cent.; in static transformers 7 per cent., and in high-tension transmission 10 per cent. I think he might very well have assumed that he could get nearly 90 per cent. in motors; a line loss of 5 per cent., even assuming that he has his insulation resistance as low as Mr. Walton put it. He could get 97 per cent. efficiency in his static transformers instead of 93; 93 per cent. in his high-tension transmission instead of 90; and 71·35 per cent. over-all efficiency instead of the 58 per cent. he assumes. If, however, he adopted a three-phase transmission, eliminating altogether his rotary converters, and taking his own efficiencies, he could still obtain 62 per cent. instead of 58; this gain may not be very great, but it is an addition of $4\frac{1}{2}$ per cent. In taking the efficiencies I have assumed, he could get 75 per cent. over-all, which will go very far towards reducing the coal bills that I am rather surprised the apostles of electric traction in this assembly are so very desirous of putting up. I cannot understand their object. Is it that they are not ready to meet the case of electrical equipment of main lines of railway? It has been done on the Continent. As Mr. Swinton has mentioned, high-tension, three-phase transmission has been in fairly successful use on the Burgdorf-Thun Railway, and a higher voltage, four times as much as on the Burgdorf-Thun line, is going to be put into use on another line next month, I hope. We shall then gain some information which will be useful, and although the station is a water-power station, I think it will dispose to a very large extent of the suggestion of separate stations at intervals of every five miles. Not only will they save the attendance which Mr. Langdon has estimated for at every one of his sub-stations, but with generating stations, as Mr. Robinson suggested, for every ten miles (which, as Mr. Swinton has justly pointed out, will mean a station loaded for a few moments and unloaded for the greater part of the hour) there will be a coal consumption in excess of the figures which have been mentioned by Mr. Walton and others. With a station every ten miles or so you might just as well retain the present locomotives—indeed, I think it would be better to do so, because you would otherwise have the assistants not only on the locomotives but at the sub-stations increased in number.

Mr. Siemens. Mr. A. SIEMENS: I would like to draw the attention of the meeting to the fact that the title of the paper is "The Supersession of the Steam by the Electric Locomotive," while the discussion has now entirely branched off into a statement of the fact that where a line of railway has eleven or twelve trains per hour, it is cheaper to run it electrically than by steam. That, I think, we all know. As Professor Forbes has already pointed out, that does not want proving. Mr. Langdon can afford to give the £330,000 for his conductors, which I think is the proper figure, and he can afford to wipe off the coal saving, which does not exist according to my idea, and still he will show a saving on such a line. But he proposes in his paper that the

Mr. Siemens

Midland Railway Company, for instance, should go on converting its line gradually fifty miles by fifty miles. Is he going to stop his Scotch expresses at Bedford to change engines when he has completed the first stage? What also is he going to do on those portions of the Midland line where there are very few trains—only two or three per hour? I think what Mr. George Westinghouse said a year ago is still absolutely true, that for suburban traffic, for any traffic which has something like ten or twelve trains an hour and upwards, it is natural that electric traction will come and that electric traction is the proper thing; but for the long main-line trains, for those parts of the railway system where there are only a few trains per hour, I do not think there is any chance yet of introducing an economical system of electric traction.

Major Cardew.

Major P. CARDEW: I think Mr. Walton and Mr. Hudlestone possibly have been misled as regards this column in Table IV., Mechanical Horse-power per Train Hour.

Mr. Langdon.

Mr. LANGDON: What I was desirous of expressing was the fact that that power was required for an hour. The table seeks to reduce everything to an hour's work, and therefore the horse-power is not intended to express exactly what is the maximum horse-power being used at any time in kilowatt-hours.

Major Cardew.

Major CARDEW: I think that one important thing to notice in the tables given in the paper is the extremely small number of miles over which each locomotive is run in the week. I find from them that on the North-Western Railway they only use each locomotive for 314 train-miles per week. Taking into consideration the time of the drivers and the firemen, the wages table comes out enormously high, and I do think a considerable economy is possible with electrical traction. Apart from the possibilities of absolutely reducing the employés on a train (which, I think, is quite within contemplation, seeing that the work to be done is reduced to practically nothing), there is much time lost by the steam locomotive before starting and after returning to the shed and during the time it is taking in water and coal. I find that in most railways they allow three-quarters of an hour for the driver and fireman before starting, and the same time after the engine has returned to the shed. All these little items mount up.

Mr. Langdon deals with the traffic as it exists. If it were really in contemplation to equip a line electrically, no doubt it would be possible to find out how the service could be best suited to the new means of working, and the tendency would be to run shorter and more frequent trains. Then, with regard to the terminal stations. With motor-cars at each end of the train, it would be possible to clear the train out of the station in very much quicker time than it takes to shunt an ordinary train with a locomotive. Much could thus be saved. In the same way the goods traffic could be arranged on much better lines for electrical working. I find that a great deal of use is now being made on the Continent of locomotives with accumulators for shunting purposes, and of course they fulfil a very desirable object. In shunting, neither power nor speed is required, but only a good horse, as it were, to drag the trucks about, and an accumulator locomotive does the job and saves a great deal of expense in the equipment of sidings. I may

Major Cardew. say that I have seen the experimental line at Buda Pesth and closely studied it, and am quite satisfied with the working. The working is admirable, and by the arrangement of the three-phase motors in series, whereby they tend to halve the speed, a great economy is effected in the starting. The same thing comes into operation in the stopping; the other motor is switched in in series, and the immediate effect is the return of power to the line. In that way the three-phase working certainly has an enormous advantage over the ordinary continuous-current system.

Mr. de Segundo. Mr. Ed. C. de Segundo: The author has attacked a subject which, in my judgment, is destined in the near future to be one of national importance, and I trust that he will accept my assurance that any remarks I may have to make on his figures are made with due deference to the opinions of one so much my senior in age and experience. First with regard to the important subject of coal consumption, Mr. Langdon has put down the figure of 3 pounds per kilowatt-hour. I have looked up some figures, and find that the consumption of coal per kilowatt-hour at the Central Station of the Dublin Railways works out to 2·1 pounds on the basis of the boilers evaporating 8 pounds per pound of coal. At the Dortmund Electricity Works it is 2·8 pounds per kilowatt generated; on the Berlin tramways it is 2·3 when using saturated steam, and with superheated steam 2·1. It must be remembered that these are test figures. I have worked out the average consumption of coal per kilowatt-hour distributed on seven of the more important central stations on the Continent, including Berlin, and I find that it works out at 4·5 pounds. This seems to point to the fact of Mr. Langdon's allowance of 3 pounds being somewhat narrow; but on the other hand one has yet to be quite sure what the load-factor will be in the circumstances in which Mr. Langdon's figures are conceived—the application of electric traction to a railway.

So far, the application of electric traction has been made under conditions which, so to speak, have been in favour of electricity—that is to say, the railway has been built for an electric railway, and there is not a sufficiently extended record of the performance of electric locomotives doing railway work under the conditions which obtain in any ordinary main line, so that we can only hazard a guess as to the nature of the load-factor which will obtain in those circumstances. Mr. Langdon has not suggested how freight-traffic is to be dealt with, and through-traffic is also an important point. Both of these contribute very largely to revenue: in fact, the revenue from freight-traffic may be 2½ times or 3 times that which is obtained from passenger-traffic, and the through-traffic may be from 45 to 50 per cent. of the whole traffic of the line. While the tendency is more and more towards short, frequent, and consequently light trains for passenger-traffic, which is admirably suited to electric traction conditions, the tendency is to increase the length, and consequently the weight, of freight-trains and to run them at longer intervals. Of course from a steam railway point of view the economic aspect of this is apparent on the face of it. But, with electric traction, it is obvious that the frequent starting of these heavy freight-trains would be a factor operating disadvantage-

ously to the economy of the steam engine at the generating station. Mr. de Segundo.
On the other hand, there is one point in favour of electricity I do not think anybody has referred to yet, and it is a very important one. We all know that only about half of the total weight of the locomotive and tender is available for tractive purposes: in the electric locomotive it is possible to apply the whole weight on the driving axles. Therefore the conclusion will be obvious that for a given tractive effort an electric locomotive need only be half the weight of a steam locomotive; and I need hardly point out the reduction in the wear and tear of the permanent way, and the consequent saving of expense of the cost of maintenance, that would result from that alone. The total absence in electric traction of "pounding," which it is impossible to avoid in the steam-locomotive, due to unbalanced piston-effort, would operate largely towards a reduction in the cost of maintenance of the permanent way. With regard to this complex question of large or small stations, I merely throw out the suggestion that should such an event be contemplated as the supersession of all steam locomotives on a line like the Midland by electric locomotives, it might be possible for the electric generating stations in some of the big cities and towns through which the railway passes to contribute in some measure to the current necessary for traffic purposes. This does not remove the objections I have raised before; but it is a possible means of reducing the capital expenditure of a railway company which contemplates such a step by making use of existing sources of supply.

With reference to the financial aspect of the question; during recent visits to the United States I have on every occasion been struck by the fact that our American cousins seem to have anticipated the fact that the future is pregnant with big events, and they have made preparations to cope with any demand that may arise. We in England have not kept pace with the times, and if we are to recover our position—I use the word advisedly—we should tear from our eyes the veil of pride and prejudice which prevents us realising our own deficiencies, and consent to learn from our neighbours, notably the United States, so that we can regain the trade we have lost, and re-establish our position of commercial supremacy amongst the nations of the world.

Mr. Frank Sprague: I have not had the time to criticise this paper, and I prefer not to do so. In considering any electric railway proposition, the practical question always stares me in the face: How is one to accomplish a result, and is it worth the while? Being possibly somewhat of an enthusiast on matters of electric traction, having given a number of years to the subject, there is no attempted solution of problems of this character, whether concerning suburban or main-line service, that does not command my interest, and I always welcome the various proposals made. Mr. Sprague.

I think the statement can be very safely made that electric railways will not be conducted on steam lines. The question is not the supersession of the steam-locomotive by the electric. The electric locomotive, rated as the steam-locomotive is rated, hardly exists. If a dynamo is considered, we wish to know the continuous output of which it is

Mr. Sprague. capable, and you gentlemen who are consulting engineers prescribe very rigid conditions of test. It must run for so many hours, at a certain speed, and a certain output; it shall have a certain capacity of excess, and so on. In short, the machine must be of permanent character, built for years of continuous service. With a stationary motor, similar requirements exist. It must be a machine which, hour after hour, shall be able to deliver a certain horse-power. On the contrary, whether it be because of a misconception of what electric railways demand, or because actuated by a somewhat unnecessary commercial instinct, the manufacturers of electric railway-motors have adopted a unique method of rating. For example, we hear of motors of 50, 75, and 150 H.P., the latter being probably amongst the largest of the units which are regularly built for railway service. These ratings mean little except that, for example, a 100 H.P. motor, railway-standard, can for an hour do 100 H.P. of work, and can stand a 50 per cent. excess of load for another ten minutes, but that is not, properly speaking, the rating of a railway-motor, as has been shown in a very interesting manner by Mr. John Lundie, who has made a special study of the kinetics and requirements of heavy electric traction.

Common sense teaches us that if the electric motor is put against the steam-locomotive, it must be a machine capable of equal continued service, and it should be rated in a general way the same as any other electric machine, on its average, as well as its special, capacity. If a small railway-motor is rated in a certain way by the hour-basis, a larger one tested under the same conditions has a fictitious and excess rating compared with the smaller one. Speaking generally, any steam or electric railway-motor should have as a measure of its rating what it can do hour after hour on active service.

Looking at Table IV., there seems to me to be a rather curious series of figures, and I quite agree with Mr. Hudleston that they are under-estimated in the matter of actual H.P. required. The method of determining train-miles per hour by multiplying the trains passing a given point by the running speed is erroneous.

What is an electric locomotive? It is simply an assemblage in which is localised a multiplicity of motors, generally four motors on four drivers, the motors being built to fit spaces which, because of the conditions under which the motor has to be operated, are restricted by wheel dimensions, wheel base, track gauge, distance between wheel seats, transom and axle, and there is a very definite limitation to the size of a motor so determined. To illustrate this fact, take, for example, one of the largest types of motors now built, rated at 160 H.P. For continuous service, and with an excess of temperature of say 75° C. above the surrounding atmosphere, it can stand about 35 kilowatts of continuous average input, or develop an average of, say, 40 H.P. available for effective traction.

I think that it is safe to say that the electric locomotive will not generally take the place of the steam-locomotive. If a steam-railway adopts electric traction, it must radically change its service, it must adopt smaller train-units, complete within themselves, operated independently, or in combinations making up longer trains. Then one

must do what is done with a hand-controlled locomotive, use a multiplicity of motors, but a greater number, distributed on the different cars or motor-car units. As soon, however, as this condition is determined, direct hand-control must be wholly or partly abandoned for general work, and the motor equipments put under a secondary control. We have to face the very practical fact that we cannot, without unnecessary restriction, compress upon a single unit the power that is necessary to do the service that is done by a steam-locomotive to-day. I of course do not agree with the last speaker that an electric motor can be built of half the weight of the steam-locomotive, that is if it is to do equal service ; but it is perfectly possible—and it is being done every day—to consolidate any number of units equipped with any required amount of power, under a common control. Mr Sprague

It is not sufficient reason for adopting electric traction to say that you can save some coal, for in many instances, especially with long trains, I question whether you can save a dollar's worth. The question of coal is a secondary one. What is all-important is capacity for increased speed, and the making of train-lengths and train-intervals subject to the will of the train-manager—in short, the ability to get and to accommodate traffic and service. I repeat that the electric locomotive on a trunk-line service has not a promising future. That service itself must be changed with change of motive power. The question reduces itself simply to the number of units which are in operation between two points. If the units are few in number, electric application is not generally to be considered. As the number increases sufficiently, then there will be a field for electric application, and then only.

Professor C. A. Carus-Wilson : There are, in my opinion, three reasons why the supersession of steam by electricity on the railways of this country will take place first, not on the main lines, but on the branch lines. Prof. Carus Wilson.

In the first place, the branch lines, or, to be more specific, the cross-country lines, as distinguished from the main lines, are the least profitable part of any great railway system. And since it is the wisdom of economic railway management to ascertain what is the least profitable part of the system, and make it, if possible, more profitable, it will always be to the interest of the great railway companies to endeavour to make the branch lines pay better, in comparison with the main lines, than they do now.

A few figures will show that the branch lines are, as a rule, far less profitable than the main lines. Taking the Board of Trade Returns for 1899, we find that for the Midland Railway the average profit per passenger train-mile is about tenpence. Taking the average number of trains per day, each way, on branch lines, as six, the profits from the passenger service per mile per day is five shillings. On a main line, on the other hand, the number of passenger trains each day might be taken at thirty each way, making the profit per mile each way twenty-five shillings, five times that of the branch lines. No doubt this is a rough estimate, but it is probably not far from the truth. Hence in considering the possible economy to be effected by the introduction of elec-

Prof. Carus-Wilson.

tricity, it will be more to the interest of the companies to endeavour to make their branch lines more profitable, than to make changes on their main lines, which are at present relatively very remunerative.

The second reason I would give for cross-country lines being dealt with first is that the cross-country lines are mainly responsible for the unpunctuality which is such a feature of our railways. I cannot attempt to establish this statement fully here to-night, but may put it briefly thus. The limited traffic on the cross-country lines does not admit the provision of a staff adequate to deal with the traffic. If the traffic were spread out over the whole working day the staff would be sufficient, but it comes all with a rush at infrequent intervals, the result being delay and unpunctuality, especially during the holiday seasons.

The infrequent service and the unpunctuality on branch lines has made cross-country travelling most unpopular, with the result that these lines are now being menaced by the tramway systems which are being projected in all parts of the country. This is a third and most powerful reason why railway managers will be obliged to consider seriously the possibilities of electricity on branch lines.

The remedy for the unpunctuality and the unpopularity of the branch lines is to be found in a more frequent service. To make proper connections with the main-line trains the service on the branch lines should be increased from an average of six trains each way per day to at least twenty-four—that is, a half-hour service instead of a train every two hours. The question is, can this be done by electricity at a profit?

To answer this question, we must know how much each mile of line contributes as profit, from its passenger service, to the general revenues of the railway, and also how much each mile must earn in order to pay its share of the fixed charges, such as maintenance of permanent way, etc. We shall probably not be far wrong in taking five shillings per day-mile as profit, and eleven shillings per day-mile as the contribution towards the fixed charges on a branch line with six trains each way per day. If, now, the running cost per passenger train-mile, that is, the locomotive charges and the guard's wages, be put at elevenpence, a traffic equal to 264 third-class passengers will be required per day-mile each way. We may, then, take this to be the traffic on a branch line as at present run by steam.

For a service of twenty-four trains per day the fixed charges and the profit per day-mile will remain the same, if the branch is to yield no more and no less profit than before, but the running charges will increase in proportion to the number of trains, and for twenty-four trains will require an income of 24×11, or 264 pence per day-mile, which, added to the fixed charge, makes 462 as the required number of third-class passengers per day-mile—that is, an increase of 75 per cent. in the traffic as compared with that required for six trains per day. The traffic must exceed this if the altered conditions of working are to yield an increased profit per mile.

Now the whole difference introduced by electrically driven trains is that the running charges are less. If motor-cars with substantial trailers are used, as on the Burgdorf-Thun line, weighing, say 50 tons,

with only a conductor and motor-man, the running charges should not exceed sixpence per train-mile. Taking this figure, we find that the number of third-class passengers required to yield the same profit and fixed-charge income per day-mile as before is 342, or an increase of 30 per cent. over the existing six-train service. Now a service of twenty-four trains each way per day might safely be relied upon to induce an increase of traffic of 30 per cent. over that of a six-train service. Anything beyond this will increase the profit per mile; thus an increase of 52 per cent. in the traffic would double the profit on the line, and an increase of 75 per cent., which with steam-driven trains would not augment the profit at all, would result in a trebling of the profits. Prof. Carus Wilson.

The right course, then, seems to be to begin on the branch lines first, to break up the system of infrequent and heavy trains hauled by steam locomotives, and substitute lighter trains running at least four times as frequently, driven by electric motor-cars. The increased traffic would be handled with far greater ease than the existing traffic,

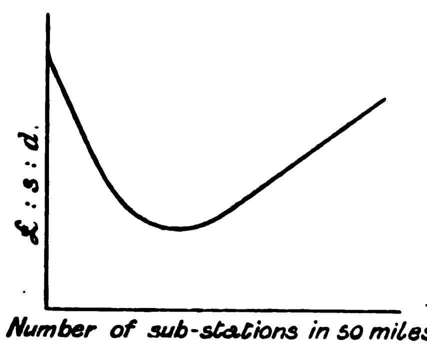


Fig. A.

because it would be more distributed over the whole day. The service would be punctual and popular, and with these advantages there is little reason to doubt that it would be profitable.

Mr. A. P. Trotter (*communicated*): Mr. Langdon has treated the problem on broad lines, and has not obscured it by detailed consideration of special points. It is quite sufficient for his purpose to assume five sub-stations, without proving that this number is better than four or six. Since he does not give data from which such proof might be established, and since I have no such data at hand, I do not propose to criticise his assumption, but to make some general observations on this point. Mr. Trotter.

If the number of sub-stations on a fifty-mile line be increased, the first cost and sub-station labour cost will be increased, the load-factor of each will be smaller, and the ohmic loss in high-pressure feeders is increased, but the much more important loss in the working conductor is reduced, and this conductor might be reduced in size and cost; and the pressure is more uniform. If the number of sub-stations be reduced, the first cost and sub-station labour cost is reduced, the load-factor will

Mr. Trotter. be greater, while the ohmic loss is increased up to the limit when no sub-stations are used. The working conductor would have to be increased to avoid excessive fall of pressure, but this case can hardly be said to have even that indifferent kind of interest known as academic. All of the factors have a monetary value, except the variation of pressure to which no such value can be easily assigned. That uniformity of pressure is important in some cases is proved by the large number of sub-stations on the Burgdorf-Thun line, but these are inexpensive and require no labour.

Given the necessary data, the conditions for minimum total cost (capital and working) may be calculated. Differential calculus will show the exact number, which is of little interest and no use. Plotting a few examples on a curve (Fig. A) will show whether the curve has a sharp or a flat bottom. It will probably be found that the bottom is flat enough to allow several external considerations, such as choice of site and neighbouring gradients, to influence the decision on the number of sub-stations.

It is probable that the number of sub-stations indicated by the mere question of cost will yield a sufficiently uniform working pressure. In tramway work the uniformity of pressure generally takes care of itself, and is the outcome of other conditions. But such conditions may perhaps show that a uniform working pressure is difficult to attain in railway work. It is said that for three-phase motors no large reduction of working pressure is safe, but for continuous current, considerations of economy and of speed allow no little latitude. It is obvious that it does not pay to level a railway through undulating country, gradients are inevitable, and they control the speed. How far will it pay to level the working pressure? If economy demands widely spaced substations, variation of pressure on the working conductor and consequent variation of speed may be a feature of the railway practice of the future. There is a good deal of difference between the maximum safe speed and the economically best speed on a well-built line. It may be an economy to space sub-stations more widely on level country, and, with suitably designed motors, to allow much larger variations of working pressure than are to be found in modern tramway practice.

Mr. Tyler. Mr. E. H. TYLER (*communicated*): I beg to share Mr. Hudleston's opinion that the electrification of main railway lines is not at present a promising field of electrical development. In any case the suburban and cross-country lines have an overwhelming claim to be treated first. But there are objections to such a sweeping change as the electrical equipment of our main lines which, I believe, have not yet been mentioned. In most European countries the railways, if not actually Government property, are under their close control, it being recognised that railways are part of the national scheme of defence.

It will not, I suppose, be pretended that an electrically equipped line is not more liable to be disabled than a steam line. The characteristic of the breakdown of a system supplied from a central source of energy is general paralysis as distinct from local injury.

There are many main lines in England that are subject to floods,

and it is obvious that it would be easier to throw an electric railway out of gear than a steam line. Mr. Tyler.

These serious considerations, as well as there being no really pressing necessity for the change, lead me to believe that the electrical equipment of our main lines is not yet within sight. The frequent reference to the Burgdorf-Thun railway as an example of main-line electrical traction is misleading. It is a full-gauge passenger and goods line, but in no sense is it a main line such as the Midland or Great Western.

Major Cardew's account of the three-phase line at Buda-Pesth was most interesting, especially his reference to the placing of the motors in series at starting and during retardation. It is not easy to see how this arrangement can return current to the line, inasmuch as the energy returned must be of the same periodicity as the line. But in any case the return of energy can only be carried down to half speed, whereas with continuous currents the regeneration can be continued until the train has nearly come to a stop.

As regards Mr. Sprague's remarks on locomotives, it is, as he points out, almost entirely overlooked that the power that can be exerted on the axles of a locomotive is strictly limited by the dimensions of the gauge and the permissible length of the wheel base. The claim of Mr. Sprague's multiple unit system is that a train composed of traction units shall have the same tractive characteristics as a single unit. Applying this test to railway trains, I would ask Mr. Sprague what he considers a railway unit to be. Obviously not one coach. If we take one half of an ordinary train to be a railway unit, and provide a locomotive of suitable weight and power to draw that unit, it follows that a full train of two units can be drawn by two locomotives coupled and controlled as one, with the same characteristics of speed and torque as the single-unit train. That appears to me to be the true solution of the principle of multiple unit control, rather than the multiplication of motor coaches. It cannot be forgotten that the adoption of motor coaches entails a wholesale scrapping of rolling stock which no company will care to face. Moreover, for many years the service on electrical lines must necessarily be a mixed one, or at all events through traffic will have to be provided for. The electrical locomotive, although not an ideal application, seems therefore to be a necessity.

Mr. H. A. HUMPHREY (*communicated*): It is admitted that the supersession of the steam by the electric locomotive is primarily one of costs. Table V. giving the cost of generation and distribution of current, is therefore of vital importance, and should have received special care; yet I regret to find it is the most disappointing part of the paper. Mr. Humphrey.

The first inspection of the table would lead one to say that the cost of generating a kilowatt-hour had been taken at 0·1574d. This, however, is not so, for the cost of oil, waste, and sundries is placed at the bottom of the page, and is not included in the total along with coal and water, although the one is as necessary to the engines as the others. Then there is no item for repairs and maintenance of the generating plant included in the table, and in seeking for this important item we

Mr. Humphrey. find that no definite figure is anywhere given, but it is dealt with along with the locomotive charges.

As a matter of fact Mr. Langdon's figures for the cost of energy from a steam-driven central station cannot be considered as constituting a safe basis for calculation. I have studied the question in this country, on the Continent, and in America, and have obtained figures at first hand from the chief engineers of large American traction stations. As a result I entirely endorse Mr. Cunningham's statement that 0·25d. per kilowatt-hour is the limit at present practically attainable in a steam-driven plant. In the case of one of the largest electrolytic plants in this country with the best modern engines and boilers and cheap coal, this figure was not quite reached even with 100 per cent. load-factor and in a year of cheap coal. The figures for the year in question were given to me by the engineer, and included no fixed charges. One of the most reliable statements of costs I know of is that given by Mr. R. W. Conant in his paper on "Cost of Electric Power for Street Railways," read before the "America Street Railway Association" in September, 1898. Mr. Conant examines and tabulates the costs from forty-four of the large traction stations in America, and he found the lowest operating cost per kilowatt-hour at the switchboard was 0·52 cents. (0·26d.). Mr. Conant describes a "standard station," and gives a standard set of figures as being the best practical. These figures come out at 0·58 cents. for operating cost and 0·405 cents. for fixed charges, making a total of 0·985 cents. (0·49d.). The load-factor is taken at $33\frac{1}{3}$ per cent., and the following statement is made:—"As this station is described its performance may seem to border on the ideal, and there is no question but that its performance is consequent on favourable circumstances, very nearly, we may say, test conditions."

To reach figures much below these a radical change is necessary, and as one attempt at the solution of the problem I would direct attention to the use of large gas-engines and Mond gas-producers. The Mond plant employs cheap bituminous slack, and recovers the ammonia in such large quantities as to allow a profit of 4s. 6d. per ton of slack, after paying all expenses in the working of the plant. As this subject is fully dealt with in my papers before the Institution of Civil Engineers (1897) and the Institution of Mechanical Engineers (Dec. 14, 1900) I need say no more about it, but desire, in concluding these remarks, to add my thanks to Mr. Langdon for his most interesting paper and the mass of information it contains.

Mr. Kenny. Mr. T. R. D. KENNY (*communicated*): It has been suggested by two or three speakers that Mr. Langdon has under-estimated the amount of power required to work his trains by taking as his tractive effort per ton the low figures given by the formula at the foot of Table IV.

I am a little surprised, however, to find that none of those who have had an opportunity of speaking have drawn attention to the fact that Mr. Langdon has provided power for only about two-thirds of the trains, which, according to his figures, must be on the section of line at one time.

His whole estimate is based on the assumption that there are only fourteen trains betw[illegible] and Bedford requiring power at the

same instant; but taking the figures given in columns 4 and 5 of Table IV., it will be seen that there must be about twenty-one trains running at once. The fourteen trains distributed on a fifty-mile section of line could not pass a given station in one hour *unless* the average speed of the trains were *fifty* miles per hour. The express trains *have* this speed, and it is correct to assume that there are only three of these trains on the fifty-mile section when three pass per hour. But take the case of the goods trains with a speed of twenty-five miles per hour. Each train takes two hours to traverse the section, and as five trains pass a given point per hour there must clearly be ten trains on the section at a time. Mr. Kenny.

Taking the other figures in columns 4 and 5 of Table IV. in the same way, we have at any time on the fifty-mile section—

3	Express passenger trains requiring			1,068	kilowatts.
3·12	Ordinary	„	„ „	425	„
5·72	Express goods		„ „	1,257	„
10	Ordinary	„	„ „	1,370	„

giving a total of 21·8 trains, which are taking 4,120 kilowatts at the axles, and—taking Mr. Langdon's over-all efficiency—requiring about 7,000 kilowatts output at the generating station as against 5,000 kilowatts allowed for in the paper.

It has been pointed out in the paper that the conclusions arrived at will not be affected by any small variations in the service, since they are based on "cost per train-mile." At first sight, then, it would seem that the 40 per cent. larger generating station entailed would only affect the magnitude of the undertaking, and be of little consequence. As a matter of fact, however, since the slowly moving trains form a larger percentage of the total on the section than in the original case, a reduction of about 9 per cent. in the figure for "K.W. per train," and of 3 per cent. in "K.W. hours per train-mile" is effected. There should also be a slight saving in the cost per kilowatt-hour due to the increased size of generating station.

These considerations will better to some extent the figure given—7·021d. per train-mile—for the electrical case in Mr. Langdon's interesting paper.

Mr. A. H. Seabrook (*communicated*): Mr. Langdon, in his highly important and interesting paper, has shown the many economical advantages that will accompany the employment of electricity upon railways, but it has probably occurred to a good many members that the advantage which will appeal most strongly to the travelling public is that of increased safety. Mr. Seabrook.

I refer to the possibility of cutting off current on any section of the line when the signal is against the train. The switches coupling up the distributing cable to the contact rail can be operated by the wire rope that works the signal.

A voltmeter, or some rough form of pressure indicator, connected across the contact and return rails, fixed immediately over each lever in the signal-box, would inform the attendant whether the switches were acting properly or not; the latter an unlikely occurrence, but

Mr. Seabrook. still to be guarded against. An indicator connected to the nearest sections of line controlled by the two neighbouring boxes would automatically inform the attendant whether his neighbours' sections were clear, whether current was in them, and if so, whether a train was passing, shown by the drop on the indicator. Practice only could determine whether the pressure should be kept off the contact rail except when a train is due; if it were not found advisable, provision could be made for the lever to work the signal alone, or both signal and switch; but there is no doubt that a great amount of leakage, especially in time of rain and snow, could be avoided. Absolutely to deprive the locomotive of its source of power when the signal is against it seems to me about the most effectual "block" system that could be put into use.

Mr. Varley. Mr. F. H. VARLEY (*communicated*): In the first place, I feel we are all much indebted to Mr. Langdon for his very valuable contribution. It seems to me that the question is purely a commercial one: Will it be more profitable to use the electric locomotive, and will the railway companies be justified in replacing their steam locomotives by the new mode of traction? To my mind, everything points in that direction.

Mr. Langdon deduces a saving of 1·922 pence per train-mile—say 21·4 per cent. Now we are informed by some of the speakers in this discussion—It is impossible to improve upon the economies now obtained in the steam locomotive, and it is quite impossible for electricity to compete with the present system. I share with most people in their admiration of the wonderful amount of applied science and mechanical skill which has improved the pioneer engine designed by Stephenson. The highest engineering mathematician's acumen, along with the mechanical engineer's constructiveness, have been brought to bear and are continually being requisitioned, and we naturally look upon the steam locomotive as a perfect piece of engineering construction and design. We are asked to compare this perfect machine with the pioneer electric locomotives, and it is not surprising that the very perfect steam locomotive, the best results of applied technical knowledge and manipulative skill, should not have it all its own way when contesting with the pioneer electric locomotive. The pioneer electric locomotive has come to stay, and will doubtless advance in development in the same way, as experience is gained by practical work. Electric railways, to compete with steam locomotive lines, will have to be worked from a different standpoint—more frequent trains and a reduction in the number of carriages per train; because long trains going long distances at long intervals carry very few passengers per train-mile. Hence arises the great waste of power in drawing heavy rolling stock, weighing say 300 to 400 tons per train, out of which not 25 per cent. is the weight of the revenue-earning passengers. In long-distance trains how often is it that they do not carry a full load, or anything like a full load. It is no unusual thing for a train to leave the terminus with 25–50 or even 65 per cent. of its passenger accommodation unoccupied. Now electric traction favours short trains despatched at more frequent intervals, with proportionate decrease of the dead weight of the rolling stock, and a larger percentage of passengers per train,

therefore a larger profit, while securing a greater amount of patronage by the greater facilities offered for travelling. Mr. Varley.

A locomotive is undoubtedly a most magnificent monument of human skill, but I defy it to make a profit by drawing hundreds of tons of rolling-stock without any passengers or goods; and yet this is practically done in a greater or less degree by all railways. Take, as one example, early workmen's suburban trains to town, crammed to overcrowding. The total weight of passengers would not exceed 47 to 50 tons; the rest of the load is that of the rolling-stock. The same train returns practically empty, still the heavy draught of the rolling-stock has to be borne by the steam-locomotive. Take the up and down journey of such train together = but 50 per cent. of its passenger accommodation.

Mr. Langdon has fully set forth the economy of stationary as against an itinerant generator. Now electric traction is specially adaptable for working short trains, which means a greater proportion of revenue-earning load to that of the unproductive load of dead weight of rolling-stock, in the dealing of which the electric system must in the long run prove far more economical.

Mr. A. Woodroffe Manton (*communicated*): As I have been interested in the design of steam-turbine-driven locomotives, which possess many advantages in common with the modern electric machines, I have been much pleased to have this valuable paper. I would like to draw attention to these special and some other mechanical advantages of the electric "engine" (and its power-house). Mr. Manton

(1) It is electrically a combination of 4, 6, or 8 motor-units, the failure of one or two not causing complete, and delaying, breakdown; further, these units may be grouped in series or parallel to meet the resistance and speed conditions. The same alternation can be made with its feeder, the power-house plant. I presume that, later, a large proportion of the gravitation-energy (now *worse than lost*) in descending long or steep gradients will be electrically recoverable in a simple manner, and returnable to the transformer stations, or for use in a motor at the time *absorbing* current.

(2) The power—that is, tractive effort—is not dependent on English load-gauge, on the maximum heating and grate surfaces, wheel diameters, or on the cylinder- and valve-chest room between the frames. The power-house steam cylinders can be efficiently jacketed, while those of the steam locomotive cannot be so assisted. There is an absence of reciprocating parts transmitting large efforts, especially of piston, connecting and eccentric rods, and "side" rods, and a replacement by parts rotating uniformly in constant direction: and the absence of connecting and eccentric rods and crank-webs allows of ample bearing surfaces.

(3) All wheels are drivers and retarders, thus the maximum axle load is much decreased, the total being much lighter for same drawbar-pull; there is, and consequently, less rail-wear (with absence of rail-shock due to want of horizontal and vertical balancing in reciprocating steam practice), greater wheel-base flexibility for high-powered machines, and *no* dependence of adhesion on inefficient "side" rods.

Mr. Manton. (4) The absence of injurious products of combustion will much increase the life, and decrease maintenance, of tunnel rails and the tunnels and bridges themselves.

(5) The greatest possible paying load to be moved on the maximum gradient of a section being independent of loading gauge and rigid wheel-base, the hour tonnage can be readily increased without fear of blocking scheduled traffic, and without resorting to enormous capital increase in acquiring land and widening ways and works.

(6) The moving parts being few with constant direction, shop repairs, renewals and maintenance should be much lessened, especially in labour; all wheel-axle-armature units may be identical and interchangeable in a short time (by electric-lift pit under rails), the difference between locomotives for various duty being made as far as possible in winding and number of axles.

For tunnels, repair-shop roads, and points liable to floods, side overhead trolley contact might be arranged. Undoubtedly much cheaper coal can be converted in the power-house (in absence of gas system even) than with the restricted fire-box locomotive design, and this coal can be more cheaply transported in large *constant* quantities to the fewer points. I think the author has rather, therefore, underestimated the power-house electromotor economies; and the Italian long-distance installation results will be very interesting (although tonnages will probably be relatively low).

With reference to cost and coal consumption, in one of last year's magazines I noted that the total costs per ton-mile, including staff and repairs, were 0·152d., 0·171d., 0·190d., on the Brooklyn Elevated, average American steam practice, and Manhattan Railway respectively (124, 138, 153 per cent.), against 0·124d. (100 per cent.) on our Liverpool Overhead Railway (electric), which latter is probably hardly now modern practice. Also it was stated that coal consumption ratio on train-mile basis was $\frac{16\text{ electric}}{41\text{ steam}}$; and that the greatest power-house and motor coal consumption per axle per hour was about 79 per cent. of best steam practice. I should be glad if the author could give more modern figures on those points, where it may be said that the traffic and resistance conditions make it comparable.

Mr. Taylor. Mr. A. M. TAYLOR (*communicated*): Though it may be considered by some as undesirable, in a paper of this nature, to criticise details, yet, unless the premises be correct, wrong conclusions may possibly be formed.

First, as to traction resistance. It may be presumed that, in measuring tractive resistance, the locomotive engineer will select as level a piece of the line as possible on which to carry out his tests, so as to avoid corrections due to grades, etc. His principal object is to determine the best performance he can get out of his locomotive in the way of speed, and the coal bill does not concern him to the same extent as it does the electrical engineer, whose principal object is to prove the economy of electric traction. We may then, perhaps, safely assume that losses due to the braking of trains when descending gradients do not enter into the determination of tractive resistance. This matter is,

of course, of less importance on railways than it is on tramways, on which values varying from 0·95 Board of Trade unit per car-mile to 1·7 B.T.U. have been experienced, due to this cause, with the same size of cars. Let us see whether it is a negligible quantity on main-line railways such as we are considering. The average running speed of Mr. Langdon's trains is about 34 miles per hour, and the tractive resistance for the level may be put at 7 lbs. per ton, taking his own figure. So long as the down gradient does not exceed $\frac{1}{320}$ or, say, 16 feet to the mile, it may be considered as compensating the up gradient; but if this limit be much exceeded it will be necessary to apply the brakes. In the route from London to Bedford and back again (100 miles) it is possible that there may be as much as 10 miles of track, in the aggregate, where the gradient exceeds $\frac{1}{160}$, giving a total drop of 320 feet, and 160 feet of this is irrecoverable lost energy. It is true that this would only amount to some 2·5 watt-hours wasted at the generating station for every ton-mile hauled; but if the gradients were much steeper, or longer, than I have assumed, the case would be very different. As it is, it adds nearly 10 per cent. to Mr. Langdon's figure of 26 watt hours per ton-mile for the tractive resistance alone. It would be interesting to know what are the actual conditions on the line in question. Mr. Raworth has given us an estimate of the additional power required for starts and stops (which is, of course, quite independent of that wasted on the gradients), so I do not propose to touch on this loss, which, however, seems to me to be low. Mr. Taylor.

Next, as regards the load-factor. Mr. Lackie has referred to the effect of the hourly variations of load on this question. But what I wish to call attention to is that the variations in the output from minute to minute have also no slight effect on the load-factor. The number of trains considered is, comparatively, quite small—only 14—and the fluctuations should be pretty considerable, especially when we remember that goods trains (with their frequent stoppages and starts) form a large proportion of the total weight to be hauled. Taking grades and everything into account, it will be surprising if, for an average load during 24 hours of 5,000 kilowatts, we do not require plant in the station of 8,000 kilowatts, while that of the rotary converters and step-down transformers will be nearly 1,000 kilowatts capacity (excluding spares). Bearing in mind that the efficiency of the engines and of the electrical plant falls off, after a certain point, more rapidly than the output diminishes, we can easily understand that, for an *average* output of 5,000 kilowatts, the all-day efficiencies cannot be sensibly higher than assumed by Mr. Langdon. I think one or two of the speakers seem to have hardly appreciated this point.

Lastly, I wish to say that I quite agree with Mr. Field's reported remarks as to the incorrect number of trains on the line; which do not, however, vitiate those of Mr. Langdon's figures which are based on train-mileage only.

Mr. REGINALD WOOD (*communicated*): The Institution is fortunate in having this important matter brought before it from within the railway companies. Mr. Wood.

The author is to be congratulated on having been content to

Mr. Wood. make only a modest claim in favour of electric traction on the great railways, as no doubt the greater economy will only be achieved when the service takes advantage of the electric supply to alter its method somewhat, and to sell current *en route*.

As I take it the author desires that the discussion should show that his paper has been critically examined, I will mention what appears to me to be an omission in his statement, but as I shall point out a remedy the objection will disappear. It does not appear to me that the author claims all the economy he might in shunting.

The omission is that no mention is made of the kinetic energy of the trains. If every train stops once in a quarter of an hour we have practically one train starting every minute. This would demand an extra 1,000 kilowatts working continuously with a corresponding 20 per cent. increase in the coal bill. There are no data in the paper to enable one to state definitely whether this extra power should be 500 or 1,500 kilowatts. But there is, of course, no doubt that this is a most important matter, though it may be of more importance in suburban traffic. Anyhow, the remedy is to brake the train by returning the energy back to the contact-rails. All that is required is to substitute for the resistance in the series-parallel control a dynamo carried on the locomotive and coupled to another dynamo suitably connected. It gives the incidental advantages of smooth starting and stopping, and the attainment of any electric pressure desired at any point. That is, the electric pressure on the locomotive is under the control of the driver. He can get excess pressure if he wants it. It also gives the advantage of saving the energy usually lost over the resistance and a great part of that lost in transmission.

This is a matter rather more than of mere detail. If there is a serious objection to absorbing the kinetic energy of the train by a brake on the locomotive in front of the train, a good case can be made out for redisposing the motive power. The description of this controller is to be found in Patent Specification 23,854 of 1894. The successful inaugurator of main-line electric supply will have earned a nation's gratitude.

Mr. Merriman. Mr. W. H. Merriman (*communicated*): There is one point which has not been mentioned in the discussion on Mr. Langdon's paper, viz., the extra precautions, impossible on a steam railway, which may be taken to ensure the safety of passengers on an electric railway. A very large percentage of the accidents which from time to time occur on steam railways is due to engine-drivers mistaking the signals or running past them when at "danger." It would be a comparatively simple matter to arrange switches in conjunction with some of the principal signals, each of which would cut out a portion of the contact-rail—say four or five train lengths—beyond the signal whenever it was at "danger." Any driver then running past a signal set against him would have his attention at once called to the fact by the slackening in speed of the train owing to the current being cut off; while, at all events at night time, the guard would also be warned of the danger by the extinction of the train-lights—presuming, as is probable, that these would be supplied with current taken direct from the contact-rail. It would, of

course, be necessary to provide against the possibility of a switch being opened before a train had reached the end of that section of the contact-rail which it controlled, in the event of the signal being raised immediately after the train had passed; but this, I think, would offer but few practical difficulties. Mr Merriman.

Mr. J. BROWN (*communicated*): I observe that engineers have recently been turning their thoughts towards the contriving of some means to avoid the delay involved in stopping "local" trains at all the stations, though only a fraction of the passengers wish to enter or alight at any one station. The plan of which I send a sketch avoids this inconvenience, provides a through train for every passenger from any station to any other, and dispels all anxiety or doubt about alighting at a wrong station or being carried past the right one. As my scheme could be worked best by electric motors, and as steam locomotives would be quite inapplicable, it falls not inappropriately within the scope of the present discussion. Mr. Brown.

The sketch (Fig. B) is intended to represent an endless railway with coaches arranged ready for starting the day's work. The coaches are

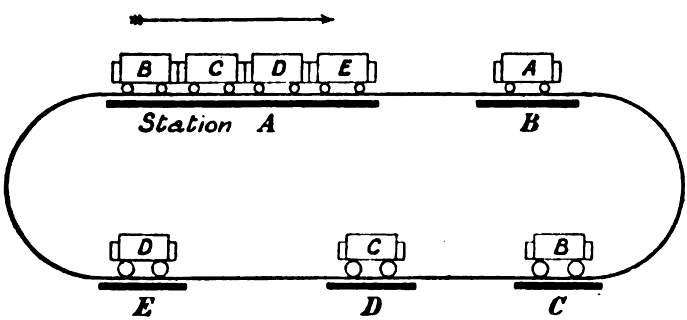


FIG. B.

of the corridor type or like American vestibule cars, so as to give a passage right through the train. Each coach has its own separate motor, and can thus run independently. A train consisting of, say, four coaches, starts from station A. As this train approaches station B, the coach A standing there is started by the driver in charge of it, and sufficient speed given to it to allow it to be picked up between stations B and C by the following train. This may seem difficult, but in reality there should be no more difficulty in picking up a coach in steady motion than there is every day in running gently up to one at rest. It is merely a question of relative motion, and with electric motors the speed would be easily and quickly controlled. If necessary, an appropriately placed signal-box would eliminate any possible risk at this point. As soon as coach A has been picked up, the driver of the train goes to its forward end and there takes control of the train, connection being made to all the other carriage motors for that purpose; or the driver of coach A takes charge of the train, and the former driver goes to the rear coach ready to slip it.

Mr. Brown. In order to allow passengers for station B to alight, the train, as it approaches B, slips its last coach marked B, which is then brought to a standstill at station B. The same process is repeated at each station. The result is a through express for every station. The passenger simply enters a carriage standing at the platform from which he departs. As soon as it is coupled to the train he looks (at his leisure) for the carriage marked for his destination, and remains there till the carriage stops, when he can alight with ease. He may not find his carriage at once, but certainly a few stations before his destination. Two or more coaches may be slipped or picked up, of course, if one is not enough.

Although here shown as applied to endless railway, the scheme is equally applicable to a railway with termini. It would be eminently suitable for the various underground electric railways recently proposed for London. From an engineering point of view several advantages accrue which may not be at first sight apparent, viz.: A much shorter station is required than in the usual method where the whole train has to be accommodated at every platform, and considerably less power would be required since only a fraction of the train has to be stopped and started. The load would also be more constant, in fact it would scarcely vary at all.

Mr. Twinberrow. Mr. J. D. TWINBERROW (*communicated*): Mr. Langdon has not credited the performance of the steam locomotive with possible improvement upon the existing practice of the Midland Railway.

This company appeared to favour the division of the traffic into numerous trains of low gross weight, worked by locomotives of considerable elegance but having small capacity. Recent practice in America and on the Continent exhibited a preference for fewer trains of greater weight worked by engines of great hauling and earning capacity; thus a standard American engine for mineral traffic was provided with boiler heating surface about 2½ times greater than that of the Midland engine for a similar purpose, and recent passenger engines in France had 70 per cent. more boiler power than the express locomotives of the English company.

The small loading gauge increased the difficulty of constructing powerful engines for working on British railways, but no important modification was involved by an addition of, say, 85 per cent., which would suffice for doubling the gross weight behind the tender; he had examined the saving per net ton-mile which would result from this modification, and was of opinion that the cost of locomotive power in working the coal traffic to London would be reduced by not less than 30 per cent. This economy could be realised without great capital expenditure by devoting the current outlay for renewals to the provision of machinery of greater earning capacity, relegating the existing stock of locomotives to shunting and branch-line service, and scrapping the lighter engines as they came to require heavy repairs.

If English engineers would be guided by what had already been accomplished elsewhere, in preference to adhering rigidly to their own precedent, they would be able to effect with certainty an economy nearly twice as important as that which the author is able to deduce from a somewhat speculative basis, with the disadvantage of a heavy

capital outlay and immense inconvenience in effecting the installation. Mr. Twinberrow.

The chief sources of economy in the application of electricity to tractive work appeared to be (*a*) saving of tare weight; (*b*) increased revenue mileage per unit of labour. A saving under the first heading could only be effected in practice by applying the motors to some of the carrying axles of the vehicles; and that under the second heading was important because the steam locomotive was not an automatic machine, and required so much attention that its attendants usually earned revenue during less than one-half of the hours of duty. It was remarkable that the author's proposal did not provide for any saving under these headings.

Mr. E. KILBURN SCOTT (*communicated*): If there were no other result of Mr. Langdon's paper, it would, in the writer's opinion, have served a useful purpose in showing the difficulties of working railways by separate electric locomotives and also the objections to a system such as the Central London when applied to main-line electric railways. Even if the pressure on the third rail were raised much higher than 500 volts, there would still be objections to converting high pressure three-phase to continuous current by transformers and *rotary converters*. Mr. Scott.

Being a synchronously running piece of apparatus, the overload capacity of the rotary converter is distinctly limited. It must be made large enough to take the maximum load under the very worst conditions without falling out of step, and naturally this load is a much higher figure than is required under average conditions. Assuming sixteen trains per day, the rotary is only likely to be giving current for a few minutes every half-hour or so, the rest of the time being spent in running round uselessly. On the other hand, the static transformers required for three-phase working will take extremely large temporary overloads if there is time to cool down between the periods of overloading, consequently comparatively small static transformers may be employed, and, when running on no load, the constant iron loss is a very small percentage compared with hysteresis, eddy, commutator, and friction losses, which are constant with the rotary converter.

Comparing the three-phase Burgdorf-Thun results with those obtained on the South Side Elevated Railway (continuous current) of Chicago, worked on the Sprague system, it has been shown by Professor Carus-Wilson that whilst the continuous-current motors use 87 per cent. of the energy required by the three-phase motors, the latter get up speed in 81 per cent. of the time required by continuous current. The reduction of energy in the continuous-current motor is due to the use of series-parallel control; but there is this most important feature in favour of three-phase, that *the maximum power input required by the motors on the Burgdorf-Thun line was only about 70 per cent. of that at Chicago.* The importance of this is seen when we consider that a reduction of the maximum power input reduces materially the size of the secondary conductors and cables, the output of the transforming apparatus, the size of the main generators, and through them the engines and boilers. Above all things, therefore, any system can be pronounced "very good" which tends to reduce maximum power input.

Mr. Scott.

If Mr. Langdon can make out so good a case on a system practically identical with the Central London, then it says much for the future for electric railway working, because there is no doubt whatever that better results can be got by adopting three-phase traction throughout. For example, the 10 per cent. loss in efficiency of rotaries would be swept away; the cost of the five substations and their equipment, given at £80,000, would be reduced to at least half; and finally, the greater proportion of the wages of the five assistant engineers, etc., at the substations, given at £5,472, or nearly 15 per cent. of the total estimated cost of generation and distribution of current, would practically disappear. Even if there were three times as many substations, the step-down transformers could be very well left to look after themselves.

The fact that certain parts of the lines are liable to be flooded, and the importance of having a standard method of picking up current puts the third rail on the sleepers, out of court altogether. It is possible to use a third rail on underground railways (although even in such situations it is questionable whether it would not be better to have smaller and less get-at-able conductors fixed overhead), but on main-line railways the difficulty of preventing people and occasionally animals getting on to the line would make the third-rail system quite too dangerous. The conductors might be put into a conduit by the side of the track, but there would be great complication at crossings and the conduit would, of course, be liable to be flooded. There is nothing for it, therefore, but to suspend the conductors on span wires immediately over the track, or else place them one above the other on poles at the side of the road. The writer thinks this latter method the best; but the point to be noticed is that as the conductors are to be overhead, it is better to have them *small in section;* in fact, it is difficult to see how anything but copper wire can be used. Now with small-section wire it is clear that to run a train the voltage must be in the nature of *thousands* rather than *hundreds* of volts.

It is, indeed, very questionable whether, at the high speeds of 50 miles an hour and upwards, the large currents required for low-voltage working could be picked up conveniently. The limit for the ordinary trolley wheel used on tramcars seems to be about 25 miles an hour, but the Siemens bow and the collecting shoe will pick up current at higher speeds; everything, however, appears to depend on the amount of current. Messrs. Siemens and Halske have recently made experiments with special bow trolleys, moving horizontally, picking up current from three conductors fixed at the side of the road. The arrangement works very well indeed, and it is found that the collection is best at the higher pressure of 10,000 volts. Having three conductors at the side of track makes an exceedingly neat arrangement, as there are no span wires and the bow contact allows plenty of variation in height for sag. Should one trolley miss the wire, the motor will still continue to run on the other phases.

With steam traction it is necessary, in order to reduce expenses, to make up trains of considerable length, so long, in fact, that it is not an unfrequent occurrence to have to pull up a train twice at a wayside platform. Now with electric traction there is no advantage in having

long trains; in fact, it is rather the reverse, as current is always on tap, as it were. Short trains run at frequent intervals, give a more even load, and as the staffs at the various stations are more regularly employed, there is a more punctual service and an improvement of conditions all round by enabling traffic and luggage to be handled with greater facility. As Prof. Carus-Wilson rightly points out, the main-line railways pay fairly well as they are; it is the short branch lines with half a dozen or so trains a day which form the non-paying portions of the railway systems. Several such lines, worked electrically from a common central station, could no doubt be made to give nearly as good results as an electric tramway, and by feeding the trunk lines more regularly would lead to an improvement of the system. Mr. Scott.

For the benefit of those speakers who seemed to think that a number of small stations are preferable to one large one, it may be mentioned that before deciding on the one station of 100,000 H.P. on the East River frontage, New York, the Engineering Board of the Manhattan Elevated Railway thoroughly considered nine different schemes, most of them being *for more than one station*. The total length of third rail on this line is 75 miles, and three-phase current is generated in the central station at 11,000 volts.

Mr. LANGDON then replied briefly, but the full text of his reply, with remarks added subsequently, is printed at the conclusion of the discussion at Local Sections meetings (see p. 218). Mr. Langdon.

The PRESIDENT: I think, gentlemen, you will have no hesitation in according to Mr. Langdon your warmest thanks for his paper The President

The vote was carried by acclamation.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Associate Member:
Alfred Henry Bland.

Associate:
Ernest Philip Alphonso Law.

Student:
Samuel Blackley.

NEWCASTLE LOCAL SECTION.

The Newcastle Local Section of the Institution of Electrical Engineers met in the Chemical Lecture Room, Durham College of Science, Newcastle on Tyne, on Monday, the 10th of December, Mr. A. W. Heaviside (Chairman) in the chair.

DISCUSSION ON MR. LANGDON'S PAPER ON "THE SUPERSESSION OF THE STEAM BY THE ELECTRIC LOCOMOTIVE." (*Read in London, November 29th,* 1900.)

Mr. Dobbie. Mr. R. S. DOBBIE: In the paper before us, the question of the money represented by the carriage of the necessary coal and water for steam locomotives over a large number of train-miles was not brought forward as strongly as it might be. It represented the transportation of a heavy dead-weight from which no income was derived. The paper confined itself, when treating of electric traction, to the use of electric locomotives. The modern tendency is to use a large number of motors; in fact, one on every axle of the train, and the cars are controlled either individually or collectively by controlling devices operating from any one point. These principles have, I believe, been put into operation in the city of Brooklyn, U.S.A., and are called by the inventor, Mr. Frank Sprague, the well-known pioneer in street railway work, "The Multiple Unit System."

Mr. Turnbull. Mr. C. TURNBULL: With regard to the statement that the coal would be saved which is at present used by locomotives standing in sidings, etc., I think that this would be considerably counterbalanced by the coal used in keeping steam on the boilers at the supply-station sufficient to meet the maximum demand, which would only come on at intervals. This point would be best dealt with by finding out what actually are the losses on locomotives from this cause. With electric trains it may be possible to develop a method of setting down and taking up passengers from moving trains. This could be done by slipping a motor-carriage off from the main train with passengers who wanted to be set down, while another motor-carriage could be run up to the main train with the passengers who wished to join it. This would mean that the main train would have to slow down a little to allow the motor-carriage to catch up to it. Possibly this disadvantage could be got over by sending the carriage on first and allowing the main train to catch up to it. I think the experiment might be tried in some form on our suburban lines. It would act somewhat in the manner of the moving platform of the Paris Exhibition.

Mr. Ralph. Mr. G. RALPH: I take it that the whole object of the proposed change to electricity as the motive power for trains is to effect economy in the working expenses. Given that is so, I should like to draw attention to another point where economy might possibly be

effected which applies equally to electric and to steam traction, and that is the saving of power by the use of roller-bearings. It is well known that these bearings reduce the bearing-friction in a very marked degree, and it is a matter of surprise to me that no experiments have been made in connection with these by the railway companies. I made some tests a few years ago on an ordinary railway-coach, first with the ordinary bearings, and then with the same coach fitted with roller-bearings. The ordinary bearings required about 45 per cent. more power to run the coach at a given speed than when the roller-bearings were fitted. (This was at a low speed, about 10 miles per hour.) It is possible that they would not stand the rough usage of railway work, and I am not prepared to state how long they would wear, though it is stated that on an electric railway at Rhode Island, U.S.A., rolling stock fitted with them ran for 21,000 miles and no appreciable wear could be detected. It seems well worth while giving them a trial. Mr. Ralph.

Mr. DOBBIE: As to the matter of roller-bearings on street cars, they certainly save a large amount of current at the moment of starting, but whether they save much energy during the running of the car I cannot speak. I have lately applied ball-bearings to motors, etc., and by a careful series of tests I found that in a particular motor of 3 H.P. with well lubricated bearings, the substitution of ball-bearings in the same motor showed an apparent gain in efficiency of only 1 per cent. I may state that ball-bearings are used by me with quite another intention than to gain efficiency. Mr. Dobbie.

Mr. G. RALPH: I quite agree with Mr. Dobbie that there is a very great reduction in the starting pull when roller-bearings are used, but in addition to this the tests which I referred to clearly proved that there was also a large saving in the actual *running* at that speed, since the watts put into the motor were 20 per cent. less, and at the same time the speed increased 25 per cent. when the roller-bearings were substituted for the ordinary brasses. When reduced to the same speed, these figures show that with ordinary bearings 45 per cent. more power is required. Mr. Ralph.

Mr. DOBBIE: The roller-bearings I have seen, and with which the starting current was undoubtedly reduced, probably as Mr. Ralph states by 30 or 40 per cent., were the Hyatt patent, the rollers of which are made of slightly flexible steel spirals, which accommodate themselves and automatically distribute the load on half the rollers more or less. Mr. Dobbie.

Mr. O. L. FALCONAR: Mr. Dobbie has mentioned something about saving dead-weight. I should like to know what would be the weight of a large electric locomotive, equal in power to some of the large N.E.R. passenger locomotives. I think it would come to a considerable amount. It occurred to me that an electric locomotive would require extra weight to provide the necessary amount of adhesion for the wheels in starting, so the saving might not be so great after all. Perhaps Mr. Holmes would give more information on the matter. The multiple-unit system is, I think, the most reasonable method for passenger trains, but I do not know how they would manage with goods trains. Mr. Falconar.

Mr. J. H. HOLMES: With regard to the multiple unit system, I am Mr. Hol

Mr. Holmes. not familiar with the details of the system, as I have not had an opportunity of seeing it in operation, but I think Mr. Dobbie has had an opportunity of seeing it and will have a better idea of the matter. The controllers at the end of each car are all worked electrically by small electric motors, arranged to work in unison with each other, so that the driver handling one controller at one end of the train, will work all the motors in the train. Of course, the system is not used with the electric locomotive, but it practically gets over the difficulty of having a heavy electric locomotive at the end of the train.

The question of roller-bearings is a very interesting one, and as we have had such very different figures submitted to us this evening, it seems to point to a difference in quality. It seems to me that a roller-bearing to be effective, should be as hard as possible so that it can keep its shape, and roll evenly on the hardened surface, and if the surface is correctly made it ought to work perfectly.

With regard to the question of changing over, I can see a difficulty in changing over to the electric locomotive from the steam locomotive. If the change would involve the same amount of confusion on our railways as we are now experiencing in Newcastle on our tramways by changing them from horse-power to electric power, I do not think we should like to make the change just at present.

Mr. Dobbie. Mr. DOBBIE: I think if it is looked into, that the principle of a spring roller is the correct one. The amount of deformation is only a few thousandths of an inch or so, and if this slight spring, which is not beyond the elastic limit, has the effect of distributing the load on a number of the rollers, it is evidently better than that the whole stress should be expended on one, or on a part of one, glass-hard unyielding piece of steel. I have seen the Hyatt roller-bearings in a train of rolls for sheet metal, where, with hardly any lubrication, they operated successfully, the bush in which they ran being of cast-iron lined with a spiral made of steel strip in the same way as the rollers themselves.

Mr. Wood. Mr. L. WOOD: If the line voltage were to be 3,000, as the engineers of a proposed Italian railway hoped to use, and feeder voltage were raised to 20,000 or even more, there would be a great saving in the cost of conductors which would be one of the heaviest items in the cost of conversion. Presumably the feeder cables would be laid in iron pipes along the permanent way, and might perhaps be advantageously made concentric with the outer conductor, bare and earthed. The current on the motors would be three-phase, which appeared to have, under some circumstances, advantages over direct current.

Mr. Heaviside. Mr. A. W. HEAVISIDE: Now that we know that high speeds up to 100 miles an hour are contemplated in electric traction, does it not become important that the form of the surfaces presented to the air in the direction of motion should be such as to offer the least resistance, as in the torpedo boat or the body of a bird?

Mr. Dobbie. Mr. DOBBIE: With reference to the high speeds mentioned, many years ago Mr. O. T. Crosby ran an electric locomotive at the rate of 120 miles an hour, and the trouble experienced in maintaining the insulation led me to patent the placing of the armatures on springs

around the axle so that vibrations were not communicated. Engines embodying this principle have been made up to 45 tons in weight to run with a heavy train about 30 miles per hour. As for such speeds as 120 miles per hour, although there is nothing impossible in them, I should be very satisfied if we could run up to London at 50 or 60 miles per hour behind an electric locomotive. Mr. Dobbie.

The matter of shaping the locomotive and, indeed, the whole train, has already had attention. Mr. Heilman has studied the shape of the front of the locomotive, and the Philadelphia and Reading Railroad has experimented with a train of cars made to present as smooth a surface as possible to prevent what naval architects call "skin-friction," and reach speeds of over 80 miles an hour.

Mr. A. W. HEAVISIDE: With regard to Mr. Turnbull's objection to large central stations of the Willesden type which he instances, it must be remembered that it is a pioneer station of its class, if one may except Ferranti's Deptford power-house (somehow Ferranti always points the way). The aim of such stations is to get a mean steady load by distributing over an area of many wants arising at different times all through the day, and Mr. Langdon's proposal is on these lines. Their success will depend on the productions gains of such a load not being swallowed up by the distribution-losses. Already much information exists as to the cost of such a load, as, for instance, in the running of alternating-current stations. Continuously energising transformers over a wide area throughout the whole year is an example, and, as regards cost, one farthing per horse-power, where coal is cheap, is not wide of the mark. Mr. Heaviside.

After the advantage of having read the criticisms of the technical press, I think that Mr. Langdon understands the principles underlying the whole thing, and though much will have to be done of an experimental character before main line railways are run electrically, the beginning has come now.

As it is growing late, I think we should close the discussion for this evening, but not without putting on record how much we thank Mr. Langdon for his paper.

The vote was carried by acclamation.

GLASGOW LOCAL SECTION.

The Glasgow Local Section of the Institution of Electrical Engineers met in the Institute Buildings, 207, Bath Street, Glasgow, on Wednesday, the 12th of December, at 8 p.m., Professor M. Maclean (Vice-Chairman) in the chair.

DISCUSSION ON MR. LANGDON'S PAPER ON "THE SUPERSESSION OF THE STEAM BY THE ELECTRIC LOCOMOTIVE." (*Read in London, November 29th, 1900.*)

The Chairman.

The CHAIRMAN, after giving a short summary of the paper, read the following communication which the Hon. Secretary of the Section (Mr. E. George Tidd) had received from Mr. Langdon :—

Mr. Langdon.

Mr. LANGDON (*communicated*): Your Section is to do me the honour of discussing my paper on the "Supersession of the Steam by the Electric Locomotive" on the evening of the 12th.

I hope it may not be thought that in submitting for the consideration of your Chairman the following observations I am attempting in any way to direct or influence the discussion, but questions having been raised on the occasion of last Thursday's discussion, at the Electrical Engineers in London, of the accuracy of the formula employed by me for obtaining the pounds tractive effort per ton, on which of course my calculations are very largely based, I am particularly anxious that this important question should, if opportunity arises, be dealt with. It may be that you may be favoured with the presence of locomotive or mechanical engineers who may be prepared, if the formula which I have used is in their opinion inadequate, to furnish a formula which they may regard as reliable. I may perhaps add that I have every confidence in the source from which I received the formula which I have used. I know it to be the result of a number of practical experiments, but I have not yet ascertained that it is applicable to goods trains. The various formulæ given do not attempt to make a distinction between goods and passenger trains. If reference is made to the diagram furnished in Fowler's "Mechanical Engineer" Pocket Book for 1900, it will be seen that there are three curves which practically coincide whilst that of Clark's deviates from these three to a very great extent.

"Molesworth" expresses Clark's as follows :—

$$6 + \cdot 009 \ V^2.$$

If this is carried out in respect of the No. 1 class of train shown in my Table IV., it will be found to result in a per train horse-power of 1,045 as against 477 by the formula which I have used; and if it is applied to No. 4 class of train, the demand will be 386 horse-power as against 183.

The difference is, as you will see, very great—indeed so great as to raise the question whether a steam locomotive, travelling at the speed of fifty miles [illegible] [illegible]ld give that horse-power output.

Mr. Langdon.

Another point commented upon was the scheme of plant. Whether it was more desirable to employ a central generating station, supplying certain sub-stations, or for the sub-sections to be provided for by, what I may perhaps call, local generating stations. I am clearly of opinion that those who argue in favour of local generating stations have not thoroughly appreciated the many advantages that attach to the central and sub-station system. If each section is to be worked by local generating services, then the machinery established and in operation must be sufficient to meet any demand that may arise on that section, which would mean an increase in cost for plant, coal and labour. It would be impossible to guarantee a fixed demand. The varying difficulties which arise in working trains at the present time will not be inseparable from trains worked electrically. Practically the same conditions will attend them. The load in the several sections will be continually varying. With a central generating system the load, as it shifts from one section to another, will not vary to any great extent the load at the central station — that will remain practically constant, depending upon the *sum total* of the load comprised within the demand of the several sections served by the central station.

Another argument in favour of the central station and sub-section system is that it will be capable of meeting, to a large extent, without stress, the varying demand consequent upon the starting of trains. At the moment when inertia is being overcome in one place, there will no doubt be a relaxation of the demand for power at another point—*i.e.*, as an instance in point, a train may be running into a station simply under the influence of momentum without any help whatever from current, whilst at another station a train may be starting and will require, for a moment of time, a large accession of current. Of course this would apply to some extent in a local section generating its own current, but it will apply very much more largely with the central station scheme—that is, as it will take a larger area, so the opportunity for equalisation will be greater.

If after reading this you should think that the matter I have taken the liberty to submit to you is of sufficient importance to read to the members at your meeting, I should feel obliged if you will do so. My great desire, of course, is to get at reliable data. In adopting the formula which forms the basis of my calculations I placed, as I still do, every confidence in it; but if it can be proved to be wrong to such an extent as materially to modify my deductions, it would be only honest that such should be done. I need hardly, however, add, that the matter is one of so great importance, that there should be no doubt as to the absolute accuracy and source of any formula that may be advanced in place of that which I have used.

Since inditing the above, I have received from the author of the formula used by me a communication thereon, copy of which I append. The data was, as you will see, collected from actual experiment in 1891, since which date the tendency due to improved lubrication, etc., should be, if anything, to reduce the tractive effort result.

Mr. Langdon.

"MIDLAND RAILWAY, LOCOMOTIVE DEPARTMENT,
"DERBY, *Dec. 10th*, 1900.

"W. LANGDON, ESQ., Derby.

"DEAR SIR,—I am sorry that I was unable to be present to hear your paper read, and offer some facts in support of the train resistance figures you submitted, for I quite agree with your conclusions on this matter.

"The formula you have adopted agrees with the results I obtained in 1891 with trains running on the Midland Railway between London and Nottingham. In all, about four hundred sets of figures were taken, and in calculating the train resistance from them, proper allowance was made for gradient acceleration, energy of revolving wheels, etc.

"The results agree closely with others obtained by Barbier, Du Bosquet, and other careful investigators. The train resistances at high speeds given by Clark and some others are quite unreasonable. The coal and water consumption of a locomotive is well known, and if such high train resistances were met with we should have to take it that locomotives sometimes run as economically, or even more so, than the best stationary compound condensing steam engines.

"Barbier found that the locomotive resistance was greater than that of the train. A comparison of the resistances I found for long trains as compared with short trains on the Midland do not support this view. He also found that four-wheeled coaches offered greater resistance than bogie stock. The figures you have quoted are about midway between those given by Barbier for the two kinds of stock.

"It must be remembered that, on the Midland, every care has been taken to use the best possible form of axle-bearing for coaches and waggons, and to lubricate the bearings, also, as perfectly as possible.

"The locomotive has also been designed, as will be seen upon reference to Mr. Johnson's presidential address, with a view to reduce frictional resistance in every way.

"It should be borne in mind that even if some steam locomotives offer greater resistance to motion per ton than do the trains they draw, such would not be the case with the electric locomotive, owing to its shape, small size as compared with its weight, evenly distributed load, and absence of all friction except that of the journals.

"The figures found for passenger trains will, I think, also apply to goods and mineral trains.

"Should you desire to do so, you may, with pleasure, make use of this letter.

"Yours sincerely,
"(*Signed*) R. M. DEELEY."

Mr. Mavor.

Mr. H. A. MAVOR said that Mr. Langdon's paper appeared to him to be a most useful one for the initiation of the discussion to which it had given rise. There were about forty-four electric railways authorised by Parliament in this country. Four of these had come into operation. It seemed on the face of it that the figures in the paper were of less importance than the subject itself. Mr. Langdon had pointed out that the prime factor was that of efficiency in the generating plant and the

electric transforming plant, and in transmission. He (the speaker) did not quite agree with Mr. Langdon. The figures in Table VII. as to the generating charges would indicate what he meant. The charges there formed a comparatively small proportion of the total estimated by Mr. Langdon. He (Mr. Mavor) thought they ought to bulk more largely. According to the paper the generating charges were somewhat less than one-quarter of the total cost for power and haulage. The whole question before them, however, ought to be looked at broadly. What was the probability with regard to the relative efficiencies of steam locomotives running on the line, each with its own boiler, and the large engine combining the power of fifty locomotives? It would be pretty evident that the large engine was probably most economical, not only in what might be called mechanical efficiency, but in general working. Seeing, however, that from the data before them up to the present it was not possible to consider the efficiency except from the standpoint of what was to be obtained from test results, the question resolved itself into one of experience. Now the experience of fuel consumption for an electric train, as compared with steam locomotives, was not available to any large extent. Some of the other items were comparable, and it would not seem unfair to draw comparisons from the experience on street railways in this country. Of the facts which had been given to them with regard to the working of street railways, the figure in connection with the renewal of plant comes out as low as 10 per cent. of the total in some cases, and as high as 20 per cent. in other cases, but in no case does it approach the 30 per cent. spoken of in Table VII. in the paper. They had much to learn, however, from experience upon this and other items, but what little experience they had went to show that the saving in electric transmission would be found in the replacing and renewal of plant even more than in the item of coal, though it might reasonably be expected that in the latter there would be a great saving. Mr. Mavor.

With regard to the position of the station, it would appear that Mr. Langdon's contention in favour of the large engine was likely to be modified by cousideration of the question in detail. Mr. Langdon had pointed out in his letter that the large engine could readily deal with a large load at various places, but did he remember that the sub-stations would have to be proportionately increased in size? With reference to the $2\frac{1}{2}$ per cent. leakage on the line allowed by Mr. Langdon, the speaker thought that such a leakage would soon dispose of the line altogether.

Concluding, he wished to say that Mr. Langdon's general conclusions seemed to him more valuable than his particular data.

Mr. DYSON agreed with the previous speaker that while Mr. Langdon's paper was a valuable one, its worth lay more in the general considerations which it contained than in the detailed figures in it. Before anything like a detailed estimate of the probable cost of electric traction could be arrived at, far more fixed data had to be got hold of than those in connection with the formula which had already been criticised. It was not necessary at present to discuss the different elements that go to make up train resistance, but the speaker thougt Mr. Dyson.

Mr. Dyson. that all engineers were agreed that the train resistance worked out on the average to a higher figure than that given by Mr. Langdon's formula. That was a point, however, regarding which locomotive engineers would be in a far better position to give facts than would electrical engineers. Speaking of the paper more generally, the question of the efficiency of the various parts as given on page 134 of this paper, this seemed to him to be not very definitely settled. The aggregate efficiency of the system, as given by Mr. Langdon, was 58 per cent., but of the items which on that page go to make up general efficiency there were two or three that seemed to him (Mr. Dyson) to be very doubtful. With regard to static transformers, their efficiency was given as 93 per cent. He thought 98 per cent. would be nearer truth. The efficiency of rotary converters was given as 90 per cent. He thought this should be 95 per cent. The loss in rails was set down as 10 per cent. It seemed to him that this could be given as less than 10 per cent. On the question of motors, Mr. Langdon had assumed that the dearest motor may have an efficiency of 85 per cent., but the speaker thought it was not the pronounced success that the old induction motor was, and 78 per cent. would be nearer the mark than the 85 per cent. allowed by Mr. Langdon. On the whole question of efficiency, as Mr. Mavor had said, one of the things to be fixed was what should be the relative efficiency of large plant, grouped in one central station, over a large number of isolated locomotives. It seemed to him that the economy of the large generating sets, when the total efficiency of the whole system showed a drop of 40 per cent., had to be accounted for by some other means than that of the economy of steam consumption of the different classes of power. Locomotive engines, running non-condensing, would not have anything like the efficiency of large stationary plant, but he questioned whether the difference would amount to 40 per cent., and he would rather be disposed to put it down at 20 per cent.

The speaker said in conclusion that he thought the first thing to be done in advancing the subject at all would be to get together some railway engineers of experience, and some men of experience in electric traction, and let them form a committee. If, as the result of the paper, this were done, even though it did not immediately lead to the adoption of any system of electric traction over steam locomotives, it would at least advance the cause of the profession, and would greatly redound to the credit of the Glasgow Section of this Institution.

Mr. Morton. Mr. DAVID HOME MORTON : You, sir, and one or two members of the Council have honoured me by inviting me to take part in the discussion of a very large subject. My contribution to-night will not, I fear, be of great service, though a few years ago I was charged with all manner of facts and figures relating to this great question of traction. Then I had the greatest difficulty in arriving at anything like reliable conclusions from the facts which were available, and I do not know that after studying Mr. Langdon's paper my position is very different, although every year facts are accumulating, which will enable us more and more to dispense with estimated figures.

If one i an advanced engineer to-day, one must

believe in the electrical transmission of power and in the internal combustion engine. I believe in the electrical transmission of power and in the internal combustion engine, but I am not lost in adoration, nor do I believe that there is but one method of generating and but one method of transmitting and distributing power. Rather I believe that, when equal skill and zeal are exercised in each case, the difference between one system and another is never so great as the retained advocates of each would have us believe. Each system has its physical limitations; those of electrical transmission are the widest, therefore electrical transmission is likely to be the most universally employed. Mr. Morton.

The retained advocates of electricity are characterised by an enthusiasm and a cheerful optimism which enable them to meet their disappointments with smiling faces. Enthusiasm is necessary; without it no great work has been carried through. Probably also optimism is necessary, because if promoters and directors knew beforehand how much the cost of their enterprise would exceed, and how much the profits would fall below the estimates, they would not often have the courage to begin, and the public would not grant them the wherewithal. The contract prices usually exceed the Parliamentary estimates, the adjusted cost is much in excess of the contract, and, even if the working expenses are all right the extra load of capital tends to absorb the anticipated gains. The investor has to be content with very small returns. He benefits chiefly as a member of the commonwealth; in the present case he has an improved means of travelling, but although he is a part proprietor of the road he has to pay his fare. Although these are facts, we engineers need not fear that enterprise will cease. Gambling is inherent to human nature.

In making these remarks, I believe you will absolve me from any ill-natured desire merely to find fault with Mr. Langdon's figures and his deductions therefrom. I think Mr. Langdon is to be congratulated for putting these figures so fully and so fairly before us, showing us his methods, telling us where he has made omissions which may affect results, and where he has deemed himself entitled to claim reductions.

When there is a large volume of parallel experience, close estimating is quite legitimate; but in new departures of magnitude, all experience teaches us that, after making our calculations, we ought to make a handsome addition to estimated capital and to add a prudent fraction to the estimated working costs.

When the estimates for the conversion of the Metropolitan Railway to electric traction are made public, we shall see how widely different are the ideas of experienced combinations of men in regard to cost or to profit. Unfortunately, few of us will know whether it is profit or cost which accounts most for those differences, and we shall have to wait a considerable time for returns of running expenses.

Very properly Mr. Langdon points out that electric traction and electric locomotives are accomplished facts, and generally we know that their use has been attended with a very fairly satisfactory measure of commercial success. Mr. Langdon, while seeking to widen the field of operation, foresees that the process of conversion must be gradual. Almost all our great trunk lines are in possession of a considerable

Mr. Morton. mileage of urban and suburban roads, and much of this mileage is in tunnels. Competition with independent railways electrically worked, with municipal electric tramways, and the pressure of public opinion in favour of improved travelling conditions, will compel the introduction of electric traction on at least some of these urban and suburban lines. And the commercial results which attend the working of these lines will determine whether or how soon electrical haulage will be tried on trunk lines.

For good reasons perhaps, Mr. Langdon has not deemed it desirable to build his estimated figures on, or to supplement these by some examination of those which are actually realised and available.

The works accomplished in this country are the City and South London Railway, the Liverpool Overhead Railway, the Waterloo and City Railway, and the Central London Railway. Of the latter two it is rather too soon to speak, although the last named will tell us most in regard to that which we seek to know. Of the two former, the City and South London Railway has been in operation since 1891 and the Liverpool Overhead Railway since 1893. I take the figures as from the accounts made up for railways in accordance with the requirements of the Board of Trade, and therefore comparable with those quoted and estimated by Mr. Langdon for the 50-mile section of Midland line, covering the same items as far as comparison can be made. Neglecting the first three and a half years, when the expenses of the pioneer line were naturally rather excessive, we find that the cost for locomotive power has averaged 6d., and that it seems now to be stationary at 5·85d. per train mile. This line, as you are aware, is worked by locomotives.

The figures of the Liverpool line are more favourable, and the cars are worked by motors on their bogie axles. The cost of locomotive power since the opening averages 3¾d., or slightly less, and has been fairly uniform since the beginning, standing now at a trifle over 4d., owing probably to the higher price of coal. The Liverpool figures may be taken as an argument in favour of motor-equipped cars instead of locomotives, but it will be wise not to lay too much stress on this. The South London line was the pioneer, the Liverpool line was built in view of all the experience gained in the other; a tunnel line has difficulties and extra expenses of its own, and the Liverpool line is what may be termed a very easy road.

The Central London has chosen locomotives, and each case must be separately considered. For short trains running on short lines with fixed terminal points motor-cars are admirably adapted, but the difficulties multiply with the length of trains and tracks, and with departures from the elementary simplicity of running cars back and forth between fixed points.

On trunk lines the tendency seems to be to seek gains and economies by pulling longer and heavier trains rather than by shortening the trains and increasing their number; while on urban lines the frequency of trains is strictly limited by the block system of working and signalling, therefore long trains must be hauled to accommodate *not* the average traffic, but the maximum which occurs only during a few hours

of the day. [illegible] trains [illegible] Central London line, and I [illegible] manager or superintendent [illegible] would be easier on the permanent way than locomotives.

Reverting to the figures taken from existing lines, I shall be told that they are not applicable: the distance between stations being short, a great part of the work consists in accelerating the trains. This is true in a measure, but observe the other conditions which [illegible] in expecting a figure much lower for these lines if Mr. Langdon's [illegible] per train-mile is right for trunk lines. The short sections represented by these small lines are well filled with trains, certainly more than one to each half-mile of double track as against three on a ten-mile section, and the gross weight of the trains in each case is only forty tons, including locomotive or motors, full complement of passengers seated and a proportion of standing [illegible] very different from the trunk-line loads, which range from two and a half to ten times greater—100 to 450 or 500 tons per train. Then, again, take the classification of trains on the trunk section. Only [illegible] per cent. to [illegible] per cent. are express, and 40 per cent. are ordinary goods and mineral trains. The amount of stopping, starting, shunting, and manœuvring which is performed by ordinary mixed goods and distributing mineral trains can at any time be studied by any one who is sufficiently interested. Apart from the not insuperable difficulties attending the putting down of installations for shunting, this is a serious item not to be lightly set aside.

An ordinary branch-line train, with a 45-ton steam locomotive, five old-fashioned coaches of ten tons each empty, seated for 300 passengers, and weighing, say, 100 to 105 tons gross, can be hauled for 6d. per train-mile, including all ordinary repairs and renewals, and we are yet without evidence that 100 tons can be hauled electrically for less. The Central London will give us the first information. The trains are heavy and comparable with urban steam lines, while the C.L.R. locomotive is 43¼ tons weight, nearly that of the tank locomotive, which is forty-five to fifty tons in full order. The tank locomotive, by the way, is on the Metropolitan Railway only 28 inches longer than the C.L.R. locomotive over the buffers.

The proper consideration of the subject is beset with difficulties, and we must wait for progressive experience, and this will give us time to study the difficulties. Of these Mr. Langdon has called attention to the most important. There is the great question of shunting at complicated junctions and sidings, and there is the struggle against the elements, snowstorms and floods.

The efficiency of the steam locomotive is an interesting though not a necessary factor in this discussion. The question seems to be essentially commercial.

Placed on the trestles, I have no hesitation in saying that the mechanical efficiency of the steam locomotive $\frac{\text{B.H.P.}}{\text{I.H.P.}}$ is 90 per cent; for a four-coupled engine a trifle less perhaps. On the track the conditions are very different from those in the testing room. Accurate dynamometer observations and cylinder indications are difficult to

Mr. Morton. obtain, and the most accomplished experimenters after taking much pains, tell us that results can only be regarded as approximations. All conditions which affect results are in a state of continual change—the train is at one time tending to lag behind, then tending to overtake the engine; then the retarding effect of curvature comes into operation, and wind pressure may actually double the required draw-bar pull under conditions when the pull would otherwise be moderate. There are also difficulties with the indicator owing to the presence of water in the steam. The most recent and most useful experiments are probably those made by Mr. W. M. Smith for Mr. Wilson Worsdell with five express locomotives on the North-Eastern Railway. The draw-bar efficiency ranged from under 60 per cent. to over 80 per cent., averaging about 65 per cent. of the I.H.P. It is fair to say that lower results have been quoted as the outcome of experiments on the Continent and in America. Sixty-five per cent. is a very fair efficiency, and it may be necessary to remind some young members of the audience that the motor axle efficiency of the electric locomotive is a very different figure from that of the draw-bar. The causes tending to lower draw-bar efficiency are present in both classes of motor, though probably in a lesser degree in the electric motor, and in the present case the loss caused by maintaining the engine and tender in motion is but a trifle more than the rate per ton required to pull the train. It could not well be expected to be less, seeing that it is the leading vehicle. The locomotive boiler is admittedly an efficient evaporator, comparing well with good stationary practice, and in the present case the water used averaged 26·5 lbs. per I.H.P., the engine in this case being non-compound. With a compound engine, taking the results of Mr. Webb's experiments on the two classes of engines, the steam consumption should be about 20 per cent. less, and if steam consumption were the first consideration a simple engine could be worked at 23 lbs. per I.H.P. These figures indicate that the steam locomotive is a fairly efficient machine, and when we remember that there is only one conversion of the energy between the boiler and the work to be done, as against four or more conversions or transmissions between the boiler and the work in an electrical system, the supersession of the steam locomotive does not look more promising in regard to net efficiency than it does in regard to net cost. And the steam department of a large central station has important losses peculiar to itself; the gross steam consumption for power delivered bears too often a very disappointing relationship to the net test consumption of the generating units.

I wish to draw your attention to the comparative expenses of the Liverpool Overhead Railway, the City and South London Railway, and a well-known local line, the Glasgow District Subway, which is worked by direct cable haulage. Do not fear that I am about to advocate this system for trunk lines. I have referred to physical limitations, and those of the cable system are perhaps the narrowest of all, though within its limits and under proper conditions the results obtained are certainly remarkable. I only bring in these figures because of the lesson they seem to teach us in regard to electrical traction.

The accounts are made up for all three lines in the same manner for B.O.T. returns, and the cost of locomotive power per train-mile stands thus :— Mr. Morton.

G.D.S.	L.O.R.	C. & S.L.R.
2·89d.	3·68d.	6·04d.
Total working expenses per train-mile—		
7·13d.	13·94d.	15·95d.
Receipts per passenger—		
1·26d.	1·96d.	1·92d.
Ratio of expenses to receipts—		
49·1 per cent.	60·8 per cent.	59·7 per cent.

If the earnings on the Glasgow District Subway per passenger were equal to the mean of the earnings of the Liverpool Overhead Railway and the City and South London lines then the ratio of expenses to receipts would be :—

G.D.S.	L.O.R.	C. & S.L.R.
32 per cent.	60·8 per cent.	59·7 per cent.

If it is objected that the trains on the Subway are lighter than those of the other lines, and that they have a smaller seating capacity, I may say that increasing the capacity of the cars would have only a most trifling effect on the results owing to the system of traction, that the car mileage is much higher than for the other lines, and that taking last year the passengers carried were :—

G.D.S.	L.O.R.	C. & S.L.R.
13,665,560	9,690,236	6,983,040

In the Subway power-station there are only two simple though high-class non-condensing engines, one in reserve, while on the other lines the engines are compound and condensing. To meet a moderate increase of traffic and a small addition to the lengths of the lines, the power plants of both electrical lines have been already largely increased and reorganised, while the existing plant on the Subway is capable of dealing with all the trains which could be put on the line under any reasonable system of working.

There are, of course, reasons for the low costs attending this example of cable haulage, and the chief reason is found in the compensation or return of energy to the driving cables by the cars which are descending grades, and by reason of the fact that cables on different tracks are virtually coupled together in the power-house ; cars descending on one line actually assist those on the other tracks. The energy stored up in the engine fly-wheels, and in the cables themselves is also called upon by the cars, so that the average and maximum powers are made less than would be possible in the absence of a compensating system.

Now there is a considerable analogy between a cable haulage and an electrical traction system, the third rail or conductor representing the traction cable, and when we have succeeded in restoring to the conductor a great part of that energy which is now dissipated by descending trains in pulverising wheels, tracks, and brake blocks, we shall have

Mr. Morton. taken an important step towards reducing the cost of electric traction. I am aware that the attention of electrical engineers has already been directed to this question. The figures which I have quoted give some indication of the advantage which may be obtained, and the works already done by electrical engineers is indication enough of their ability to solve this problem.

Prof. Barr. Professor BARR expressed his dissent from Mr. Morton's opinion that electrical engineers were characteristically too sanguine and too optimistic, though he admitted that there had been a time when some of them were so. He instanced the case of an electrical engineer whom he had met at the Glasgow Gas Exhibition of 1881, and who in the course of conversation had assured him that incandescent electric light would be in use in every house within a year from that time.

Continuing, the speaker said that he thought that the question before them of the supersession of steam by electricity for locomotives, was a problem in which the arguments for Electricity were more favourable than in many other cases to which it had already been successfully introduced. There could be no doubt that in a great measure the economy obtainable in stations for lighting and tramways was limited by the nature of the load-factor. There was no case equal to a large main line for constancy of load-factor. He (the speaker) did not think that Mr. Langdon had in that matter done his case at all justice in his paper. He thought that it would be found that the load-factor would come out very much more constant than is stated in the table in the paper, as would be seen if the number of trains on the whole line, instead of the number passing a given point during a given interval of time, were considered.

It was a pity that they had not more results before them, but he thought that as far as could be judged from appearances, electric traction would not come in through the calculation of probable economy alone nor even mainly, but in virtue of the obvious advantages the system possessed in special cases over steam haulage. The underground railways, he said, would some day be compelled, by public opinion, to use electric traction, and he thought also that the time was not far distant when they should have a great deal more objection to the running of locomotives in the vicinity of cities than was yet manifest. A great deal of the smoke nuisance in suburban districts had been proved in many instances to be caused by locomotives.

Concluding, he said that he had no doubt that under the compulsion of a growing public opinion on the subject, the time was coming when they should see a very large development of electric traction even on main lines, and he personally would be very glad to see electricity substituted for the present objectionable locomotive in the cases to which he had referred.

Mr. Lackie. Mr. W. W. LACKIE: None can doubt Mr. Langdon's remark that the consideration of this subject can only be attended with good. It is a large problem to tackle, but his conclusions and results are most satisfactory. I would, however, like to draw attention to some of the figures and conclusions come to by him. It is shown that the maximum number of trains per hour varies from 19 to 7. If this is the case, it

Mr. Lackie.

means that he cannot have, as he states, an absolutely constant load and station output for every hour of the day. His figures would at first lead one to believe that in this railway scheme he would have a load-factor of 100 per cent. The kilowatts wanted for the 19 trains I make out to be 8,650, and for the 7 trains 2,080, or less than one-third of the maximum load. Mr. Langdon takes an average of 5,000 kilowatts, but he strikes his average by taking the maximum average in every case. He says that in each train of this class he has taken it as a loaded train, whereas some would certainly be light trains. He also accords to each their full merit of speed. The average number of trains passing Luton is 11·9, and passing Harpenden 12·4. He has taken an average of 14 trains per hour. 5,000 kilowatts will not drive the 19 trains at their full speed. The units generated per annum he makes out would be 43,800,000. That is taking 5,000 kilowatts for 24 hours and 365 days. I think the fairer way of getting the total units likely to be generated would be to take Tables II. and III. on pages 129 and 130, and work out the units per hour throughout the 24 hours. If this be done, the figure is more likely to be 39,000,000. If the units are less, and as low as I calculate them to be, it affects the cost per kilowatt-hour, and consequently per train-mile, by fully 10 per cent. in every case except the coal bill. All the other items making up the cost per kilowatt-hour are per train-mile, are standing charges, and are quite independent of units generated. As a matter of fact the load-factor, on such a scheme as is before us, will only work out at something like 65 per cent., *i.e.*, the ratio of the units which would be generated, if the maximum load 8,650 kilowatts remains on for 24 hours throughout the 365 days, and the actual units likely to be generated. Further, I do not think that the engineer in the station is likely to know when the maximum load would come on, and consequently he would require to keep the maximum power running, in case it did come on. The annual coal-bill of the Liverpool Overhead Railway, including carting and ashes, is stated as 0·118 per kilowatt-hour. Even if allowance is made for a lower number of units per annum, the cost per train-mile is fully 1¼d. in favour of electric driving.

There is one other thing which I would like further explanation of, and that is the 2½ per cent. allowed for leakage. It would appear that the insulation per mile of the line was only something like 180 ohms.

Towards the end of the paper Mr. Langdon discusses the adoption of electric traction on small branch-railways, and incidental use of gas plant. I quite agree with him that there is a very large field for the use of this, as the cost per horse-power hour by using gas on plant not exceeding 400 H.P. is one quarter of that when using coal and steam. Further, in the small branch-lines, the whole plant could be shut down, as the stations are during the night. For small plant, therefore, gas engines would be preferable. Fluctuations in pressure are not of serious account.

Mr. Field.

Mr. M. B. FIELD: I read Mr. Langdon's paper through several times, and each time I did so the more dissatisfied I was with the results he arrives at. In my opinion Mr. Langdon's figures from beginning to end are at fault. He has attempted to imitate the traffic over a

Mr. Field. certain section of the Midland Railway, but, in doing so, has assumed only about half the trains there should be, and has made up his total in quite a different proportion from that existing over this section. Next, his choice of the electric system appears to me to be not necessarily the best. The items of the cost per train-mile deduced by him appear to me very doubtful; and lastly, the comparison itself of the assumed electric system with the existing system seems unfair, as the two systems are not at all on the same basis. These points I propose to go into a little more fully. Looking at Table IV., I am inclined to think that a great mistake has been made. The second and third columns show the number of trains of different classes that pass Luton and Harpenden in 24 hours, giving averages of 11·9 and 12·4 passing per hour the two places respectively. On page 133 the author tells us he has taken 14 trains passing per hour a particular point, and has apportioned them :—

3	trains,	speed	50	miles	per hour.
2	„	„	32	„	„
4	„	„	35	„	„
5	„	„	25	„	„

These are tabulated in column 4 of Table IV. The author then tabulates in columns 7, 8, 9, and 10, the load, tractive effort, mechanical horse-power, and equivalent in kilowatts corresponding to each train. Columns 11 and 12, headed "Total Mechanical H.P. and K.W.," are obtained by multiplying the number of trains of each class by the corresponding figures in columns 9 and 10, *i.e.*, the mechanical horse-power and kilowatts per train. Evidently this can *only* give the total kilowatts required to propel all the trains that pass a given point in one hour.

But later on, on page 135, the author assumes that there are only 14 trains on the 50-mile route, viz., 3 to each of four sections, and 2 to the fifth section.

But with 14 trains going at an average speed less than 50 miles per hour there clearly cannot be 14 trains passing a particular point of the 50-mile route every hour. Refer again to Table IV., and assume trains of classes 1, 2, 3, and 4 run at their respective speeds for 24 hours *without stopping*, backwards and forwards over the 50-mile course :—

In 24 hours	2·8	trains	Class	1	would	pass	Harpenden	67 times.
„	2·66	„	„	2	„	„	„	41 „
„	4·66	„	„	3	„	„	„	78 „
„	9·3	„	„	4	„	„	„	111 „
	19·42							297

Therefore in 24 hours with 19·4 trains there would pass Harpenden 297 trains, giving an average of 12·4 trains per hour. And assuming 14 trains pass a given point per hour, and the train stops at stations, there could not be less than 25 to 30 trains at one time on the 50-mile route, or approximately double that allowed for in the paper. This ·ould not be of great consequence had the author taken the right pro-

portions of the different classes of train, but he has not done so. The proportions are :— Mr. Field.

(1)	3	as against	3
(2)	2·85	„	2
(3)	5	„	4
(4)	10	„	5

This appears to me to throw the whole Table IV. and subsequent calculations wrong. The point is that the author appears to have assumed that because, say, 5 trains of class 4 pass a given point of the route per hour, that there are only 5 trains altogether of this class on the route. This cannot be, unless they run at 50 miles per hour. Before leaving Table IV. I would like to ask what is really meant by so much horse-power *per hour*, or kilowatts *per hour*, horse-power per train-*hour*, *hourly* output of so many kilowatts; and on page 133, why, because 14 trains pass per hour a particular place where there are four roads, is one justified in saying that there are 3·5 trains per mile per hour, per line of metals? This I cannot follow.

I will not stop to criticise the tractive effort assumed by the author for the different trains, though it appears to me to be very inadequate; but will assume that 5,000 kilowatts will work 14 trains travelling at their respective speeds a total of 480 miles per hour, backwards and forwards on the 50-mile route.

Look at Table VII. We can divide the total cost into three main heads—

Generating and Distributing per train mile		2·11d.
Locomotive drivers and assistants	„	2·65
Renewal of machinery, cable, etc.	„	2·26
		7·02d.

2·65 pence per train-mile for driver and assistants is equivalent to 0·25 pence per kilowatt-hour, see Table VII. Now suppose these men work *only* 8 hours per day, *i.e.*, for the 14 locomotives, we should want 84 men who get 0·25 pence per kilowatt-hour. Compare this with Table V., the wages paid to the men looking after the machinery supplying these 14 locomotives. There are :—

	1	Chief Engineer at	£200 per annum
	1	Assist. do. „	250 „ „
	5	Do. do. (for sub-stations) at	200 „ „
	3	Switch men at	150 „ „
?	15	Do. „	140 „ „
	1	Clerk „	120 „ „
	7	Engine Attendants at	40s. per week
	7	Do. do. „	35s. „ „
	12	Stokers „	30s. „ „
	15	Labourers „	22s. „ „
		Other Sub-station wages at ...	£2,400 per annum

Mr. Field. There are wages allowed for at least 84 men here, and by Table V. the total cost of these wages per kilowatt-hour comes out 0·0544d., whereas for the 84 locomotive attendants Mr. Langdon gives in Table VII. 0·254 pence per kilowatt-hour, or nearly five times as much. If we are to account for this by the fact that other locomotives are standing idle ready for use, it means practically that 4 squads are idle for every one on the move, and this for every hour of the day and night. This is incredible. Look at this another way. Each locomotive travels $\frac{479}{14}$—34 miles per hour, and each squad (if only working 8 hours per day) would cover 1,900 miles per week. The combined wage of the two locomotive men would be 80s. per week, which comes out at ½d. per train-mile instead of 2·65 pence; hence it is that more than 4 squads are paid for each one that is on the move. In the footnote on page 139 Mr. Langdon shows that 2·65 pence corresponds to a daily mileage of from 45 to 58 miles; how, then, can he apply it to his case where his hourly mileage is 34 per running locomotive, or, if we take two idle locomotives for every one that is moving, a daily mileage of 270?—unless he can show that in the case of the electric locomotive some abnormally great expense, which is not at all obvious, is likely to come in here. Surely some explanation of this figure is needed; it cannot include cleaners, coal men, tube men, repairs, etc., for these are given as independent items in Table VI., but it is hardly possible that it stands for the wages of drivers and assistants alone. In any case it appears to me that a comparison of costs worked out for a more or less hypothetical case where the daily mileage per locomotive would lie between 270 and 300 with those of the Midland Railway, where from Table I. we see the mileage for 1899 was but 52 per locomotive per day, cannot be of very great value. Now come to the third heading of the total cost, viz., repair and renewal of machinery, etc., at 2·26d. per train-mile. The author assumes 2d. per mile for the renewals and repairs of everything except cables and third-rail, this being one-fifth less than with steam, because he says the wear and tear of stationary engines or motors cannot possibly be as great as with steam locomotives; and with these words he practically dismisses an item which is one-third of the total cost per train-mile. Now we have to compare the wear and tear of stationary engines, boilers, economisers, condensers, exciters, switchboards, buildings, sub-stations, transformers, rotaries, cranes, chimney stacks, and electric locomotives, with that for steam locomotives alone; and I must say that the above conclusion does not appear at all obvious to me.

According to my reckoning the sum of 2·26d. per train-mile comes out at about 6¾ per cent. of what seems to me would be the first cost of a complete 10,000-kilowatt generating station, plant, with locomotives; if, however, more spare plant be allowed for, as seems to me to be desirable, the sum allowed for depreciation, renewals, etc., would come to something like 5 per cent. of first cost, which, since it has to include the wear and tear on the third rail besides the heavy expenses for upkeep of locomotives, certainly appears insufficient. Although Mr. Langdon gives the total average saving effected by the employment

of electricity on page 141 to six significant figures, I notice that on page 147 he is willing to knock off 20 per cent. of this for shunting operations, and to duplicate the generating units if necessary with a corresponding increase of his figures. Mr. Field.

I would like now to add a few words on the electrical side of the question. Even here the question is never raised as to whether the assumed system is the best for the purpose. In my opinion it is questionable whether the proper system would not be to employ three-phase motors on the locomotives. The distance between stations is great; the speed between stations might be maintained constant and the same for each class of train; then, would it not then be feasible to have a 15,000-volt transmission line (if abroad, an overhead line) and feed into the line through static transformers at intervals, transforming from 15,000 to 1,000 or 2,000 volts; the overhead trolley or other overhead contact system to be fed at this pressure, and the locomotive motors to be wound likewise for this pressure? If this system were feasible, the sub-stations with their engineers and rotating machinery would be replaced by transformer chambers requiring no attendants, the loss in the rotaries reckoned here at 10 per cent. would be eliminated, the rail loss of 10 per cent. would be replaced by an overhead conductor loss of, say, 4 per cent., and the losses in transmission line and transformers would be reduced. In fact, I think an improvement of 20 per cent. on Mr. Langdon's overall efficiency would be possible. Continuous-current series motors with rotaries are assuredly the right thing for heavy city traffic, where distances between stations are short and speeds are proportionately high. In such cases the most economical procedure is rapidly to accelerate up to maximum speed, and then, if possible, to cut off current entirely. In such cases the speed would be never constant for a moment, and three-phase motors would therefore be most uneconomical and inefficient. Where, however, long stretches intervene between stations, constant speed may be maintained, and three-phase motors will be equally efficient as series motors. Three-phase motors would, of course, be less efficient in fogs where the speed would have to be reduced; special low-speed locomotives would be built for goods trains, and for use where shunting was carried on extensively. The adoption of three-phase motors in the locomotives certainly seems to me to merit discussion, but Mr. Langdon does not refer to it at all.

Mr. Patrick M. Barnett said that he had come to listen and not to speak, but he had listened with a great deal of pleasure to the discussion. He thought that it would be a waste of their time for him to make any remarks on the subject, more especially as he was neither a mechanical nor an electrical engineer. He was, however, interested in the question before them very much, and he would like to put one or two questions. Mr. Barnett.

In regard to electric traction by a conducting-rail, he could easily understand that in a subway or tunnel such an arrangement might be quite suitable and proper, but he would like to know how it would be possible in country districts. They knew that in this country there were a great many level-crossings in connection with railways, and the continuous rail could not be carried over these crossings. He would like to

Mr. Barnett. know how they were going to manage this withou breaking the electrical connection.

Another point that he would like information upon was what effect electric traction would have in the case of those working on the line. What would its effect be on the surfacemen or on the train if a crowbar was laid across the conducting and running rails?

Continuing, the speaker gave an account of a visit to a railway in France worked by an electric motor and accumulators.

Mr. McIntosh. Mr. J. F. McIntosh expressed the pleasure he had in being present and hearing the discussion, and said that, like the previous speaker, he was a mechanical, not an electrical, engineer, and would have preferred to be a listener only. With regard to the question that Mr. Barnett had asked, he thought that if Mr. Barnett had gone to the Paris and Orleans Electric Railway he would have got the information he desired. The speaker had been curious to get information regarding this very point, namely, as to how an electric engine was to get over a crossing, or do shunting, without interruption of the transmission of power. He had had the pleasure of seeing how this was done, and also of travelling on an electric locomotive which took the Bordeaux express from Quai d'Orsay to Austerlitz, a distance of 2½ miles. Mr, Barnett would have found that the power was transmitted from the ground rail to the roof of the tunnel in shunting, and that a rail was placed on the roof, so that immediately the engine leaves one line for another, it takes the power from the top.

Another point that he would like to speak about was the trouble likely to arise in the winter time in connection with snow wreaths covering the power-rail. He had been puzzled to know how snow would affect the transmission of the current from the rail to the locomotive. But he had discovered that this also could be got over in the same way as they do in France, and that was in cuttings where there were likely to be snow wreaths the rail could be elevated, so that it could be out of the reach of snow. On the electric railway in France, to which he had already referred, he could assure them that it was a very interesting experiment to him to step on to an engine which had no steam; but when the driver got the signal to start he pulled the cord, and the whistle blew in the very same way as an ordinary locomotive. This whistle he found was worked by air, a small dynamo working a small Westinghouse pump. Before the train had gone very far they were going at a speed of from 20 to 25 miles per hour, and it seemed that this speed could very easily be increased. What had struck him very much was that an incline seemed to have no effect upon the electric locomotive, and he was very much impressed with it.

Concluding, the speaker said that with reference to the discussion, he thought they had perhaps been going just a little too far ahead. They were all aware of the necessity of the supersession of steam by electricity. They knew that the coals in this country could not last for ever. He was therefore more inclined to thank Mr. Langdon than to criticise his paper, seeing that he had given them the opportunity of discussing what would be the best methods to adopt in electric traction; and without beginning to calculate the cost or expense,

he thought they ought to keep in mind the necessity of having the lines worked by electricity, and to consider the best way by which this could be done. Mr. McIntosh.

Mr. SAM MAVOR: In Table VII. the largest item by far is for drivers and assistants, and it is not clear that so high an allowance as 2·65 pence per mile is necessary. I have learned from Mr. McIntosh, of the Caledonian Railway, that a fair day's mileage for a driver is from 60 to 120 miles, but sometimes 130 miles is performed. Assuming an average of 90 miles per day, and a cost of 15s. per day for driver and assistant, the rate per mile would be 2 pence. There can be no doubt that a greater daily mileage would be realised from the drivers with electric than with steam-locomotives. The electric locomotive depôts being close to the terminal stations instead of several miles distant from them, as is frequently the case, would save drivers' time both at the beginning and end of the day's work, and the time of the driver going to or from the depôts or water towers for coal and water would be saved with the electric locomotive. Mr. Mavor.

The section of the line upon which the paper is based is favourable to the case for electric driving, as the load-factor is good, and it is doubtful whether Mr. Langdon's assumption would be realised that approximately similar results would be obtained on branch-lines. The splitting up of trains and giving more frequent service, although it improved the load-factor, would largely increase the cost for drivers. It is interesting to note in this connection that the tendency on main-line trains is to increase their length and weight. Such luxuries as dining-cars, now so common on express trains, could not be afforded unless trains were long, and the additional space provided for the comfort of passengers has led to the use of the larger and heavier coaches now employed. The section of line considered in the paper is approximately level, but on other sections of the Midland, and on the London and North-Western, and on the Scottish lines, steep gradients have to be faced, and this, coupled with the increasing weight of express trains, would involve the use of electric locomotives of much greater power than those anticipated in the paper.

Mr. W. PICKERSGILL: In computing the total power required to work the train on the given line between St. Pancras and Bedford, Mr. Langdon has taken for the basis of his calculation the number of trains passing a given point in a given time. I am of opinion that a more accurate result of the power required could have been obtained if the maximum load to be hauled at an average speed at one time had been taken for the whole of the 50-mile line. Generally speaking, the maximum number of trains that can be put upon a length of line depends upon the number of block sections, and assuming that there are 10 block sections between given termini, it would be possible to get as many as 40 trains in a 50-mile section of four rails at the same time. This, however, is very improbable, but I am of opinion that during holiday seasons or specially heavy traffic there would be many more trains on the section than 14, which is the number that Mr. Langdon bases his power upon. If this were so, it would be found that in order to provide for the maximum demand, as also to insure Mr. Pickersgill.

Mr. Pickersgill.

against breakdowns, and have duplicate plant for repairs, a larger installation would be required and consequently increase of capital, which would have the effect of increasing the standing charges and the cost per train-mile.

I am of opinion that the formulæ adopted by Mr. Langdon for the traction effort for moderately high speeds, viz., $3 + \frac{V^2}{250}$ pounds per ton is not far from being correct, but I find that no allowance has been made for the resistance due to gravity, curves, or head and side winds which offer very serious resistance to the passage of trains, especially when running at high speeds.

If the data as to the cost of working a train-mile by steam locomotive includes the expenses due to piloting, light running, and shunting, and other work not classed as train-miles, then the comparison as to the cost of working by electricity, not being on the same basis, is fallacious, and it would be advisable to get a true comparison before seriously discussing the question.

I should also expect that the average cost per train-mile for steam locomotive also includes a large amount of heavy suburban and local goods working through some of the busy districts on the Midland line, and the cost of working these trains would no doubt be very much higher than the cost for working trains on the main line. If these trains, which can hardly be considered as main-line trains, and also the cost of working them, were eliminated, it would no doubt have the effect of materially reducing the cost per train-mile for the section under consideration.

I observe that Mr. Langdon allows a sum of £50,000 per annum for the whole of the shunting on the Midland Railway, but I am of opinion from experience that this sum must have been far too little to cover the cost, considering the magnitude of the work on the Midland Railway, to say nothing of the piloting and light miles and other work not included in train-miles.

Mr. Yorke.

Mr. R. F. YORKE (*communicated*): I have only just received to-day a complete printed form of the paper, and am therefore rather handicapped in entering into a discussion on it at such short notice.

It has been printed by the electrical papers, but in some cases errors have crept in, such as that concerning the pressure which was stated to be 100,000 volts instead of 10,000; and the paper itself for publishing purposes has been split up into two different parts. I should like to take this opportunity of appealing to our electrical press, whether they could not in important papers of this kind print the whole in one issue. It is most tantalising, on arriving at the interesting part, to have to wait another week for the conclusion.

It is a great pleasure to me to see my former chief of the electrical department, Midland Railway, again to the front. He was at the front in the early days of electric lighting, not only as regards the electric lighting of the Company's hotels, but in lighting the goods and shunting yards, which entailed a subsequent diminution in the risk to life, and many other contingent advantages. Mr. Langdon is now again to the fore with his present proposal for substituting electric locomotives in ace of those worked by steam.

Mr. Yorke

The present time does not appear opportune for discussing engineering details. Provided the railway companies accept the principle, there is any amount of electrical engineering talent in this country successfully to evolve the best system to be adopted. This system must be applicable to all the railways, so that such errors as those which occurred in connection with the broad and narrow gauge may be avoided.

There are, however, one or two general points which I should like to notice. The first is this: that assuming electric working be introduced, the public will certainly expect that trains will be run at an increased speed. It will be quite possible to do this, using the existing weight of rails, as the injurious knocking action of the reciprocating engine will be absent.

The other point I note is that the author has taken as the basis of calculation the working of an important main line; but how do the figures work out for branch lines with only a comparatively light passenger traffic? Will it be possible to work these lines also economically? I am interested in this question, as I have lately had occasion to lay a proposal before one of the Scottish railways for working a short line electrically by means of water power, and the question to be decided was whether, in view of the small traffic, it would pay to put down the necessary plant. Even supposing that the railway companies do not accept the proposal in its entirety, I still think that our Highland railways would benefit by the adoption of auxiliary electric locomotives for working their steep gradients. Where these heavy gradients exist, water power is nearly always present. Take the Highland Railway, for instance, with the steep climb from Struan to the top of the Grampians. The river Garry would furnish any amount of power for auxiliary locomotives.

Again, on the West Highland Railway, with the long climb from Spean Bridge up to the summit at Corrour, the water power of the Spean Valley could provide some 10,000 H.P., which would be greatly in excess of what would be required.

I agree with the author on one of the points on which he lays stress, namely:—that the new system can be introduced by degrees, and when it has been proved to be a success, further great extensions will necessarily follow.

At the conclusion of the meeting, Mr. H. A. Mavor moved "that Colonel R. E. Crompton and his gallant boys, who are being officially welcomed back from the front in London by our Parent Institution on Tuesday next, be accorded a hearty vote of congratulation by this Section, and that we send them our sympathetic wishes on the conclusion of their patriotic and arduous campaign."

This was duly seconded, and was carried with acclamation.

REPLY OF MR. LANGDON TO THE DISCUSSION ON HIS PAPER: "THE SUPERSESSION OF THE STEAM BY THE ELECTRIC LOCOMOTIVE." (*London, December 6th, 1900.*)

Mr. Langdon.

Mr. W. LANGDON, in reply[1]: It is perhaps unnecessary that I should say that the paper which I have submitted for your consideration is practically the outcome of the success that has attended the recent application of electricity to the movement of railway trains. That which has been done has shown the practicability of the application of that power to the purpose, and naturally the question arises whether any and what economy might be expected to attend its use, supposing it should be found applicable to the same purpose in connection with our large railway systems. I have been attacked because I have brought this paper before the members of the Institution of Electrical Engineers. Well, it naturally appeared to me that it was a question which intimately affected the Institution of Electrical Engineers, and that perhaps no better body in the kingdom could be found to discuss its merits or its de-merits. It is not improbable that some of my deductions may be erroneous, although I have endeavoured in considering the subject to deal with it in an unbiased manner and as fairly as possible. I do not suppose that if electrical energy is applied, as undoubtedly it will be applied some day, to the movement of the traffic of the main lines of railway of this kingdom it will be in my time; I may see something of it, but I shall not see its general application. I have therefore no personal interest in the subject beyond the advancement of that which I believe will prove of vast interest to the community at large.

Professor Forbes has stated it is a paper which ought to have been brought forward somewhere else, and he has in a measure criticised my deductions. As I have stated in the paper, the subject has necessarily been dealt with by me in an abstract manner. The purpose of the paper has been to evolve, if possible, some result as to the economy and other advantages that might attend the use of electricity as applied to our large railway-services, and it certainly appeared to me that there was much more probability of a successful issue if applied on portions of line where the line was considered to be full of trains and where there was a great deal of work doing, than if a section of line less used were selected. It was for that reason that I took the London and Bedford section of line. I do not know if I am wrong in the tractive effort formula which I have employed, but I do know that it is derived from absolute experiments. I am not, however, prepared to say if it embraces actual results obtained from goods and mineral trains, but I have every confidence in its accuracy with respect to all kinds of passenger trains.

Mr. Mark Robinson, on the occasion of the last meeting, raised the

[1] The first portion of this reply was given verbally at the Ordinary General Meeting in London, on Thursday, December 6th (see p. 193).

question whether it was preferable to work a section of line from a central generating station serving several substations or to work it from a number of local generating stations. I feel quite satisfied that his remarks were *bonâ fide*, that they were not at all influenced by any personal desire with respect to the type or capacity of stationary engine which should be employed. Messrs. Willans and Robinson had in the Paris Exhibition an engine of some 1,400 H.P., I think, and I have no doubt that if they were called upon to produce an engine of still greater capacity it would be quite within their power to do so ; but that which has, I think, escaped the attention or consideration of several who have taken part in this discussion has been the fact that with a central generating station serving substations you are able to transfer the sectional load from one section to another without materially altering that at the generating station. Mr. Langdon.

Mr. Raworth has called attention to the fact that although I had taken 14 trains as the average number of trains passing over the section of line in one hour, one of the tables produced in the paper showed that as many as 19 trains were present during one of the 24 hours. That is a fact, but Mr. Raworth omitted to say that during other hours the number was considerably less, the minimum being in one hour as few as seven. That shows how the load will vary. There will be a continually varying sectional load so far as the subsections themselves are concerned, but the load at the central generating station will be practically continuous. If you have to establish local generating stations, of course you have to make provision for the maximum number of trains that would be in that section, and your power must always be there in order to give the possible maximum output. That means you would have to increase your plant, and as your load would not be constant as it is at the large central generating station, you could not effect the same economy especially with regard to your coal bill. I have since the last meeting gone into some figures with respect to the establishment of these local stations, and I make the primary outlay £430,000, instead of £470,000 for the central scheme, while the working expenses come out at 0·8233 per kilowatt as against 0·6726 for the central generating station, and per train-mile at 7·160 against 7·021. If we add to that interest at 3½ per cent. on the outlay, we get this result : 0·9223 per kilowatt for the local generating stations as against 0·7716 for the central station scheme ; and 8·0208 as against 7.662 per train-mile. In making that calculation I have taken coal at 4 lbs. per kilowatt, because it seems to me quite clear that you could not maintain 3 lbs. with so varying a load.

The subject as to whether a section of line could be worked more economically or with greater advantage by a central station feeding so many substations, or whether the line should be divided into sections, each section being provided with its own generating plant, is one which would naturally command careful consideration at the hands of any one who might be called upon to consider its practical application.

It has been stated that my allowance of 3 lbs. of coal per kilowatt is insufficient. Mr. Cunningham, on the occasion of the last meeting, quoted 3·6 lbs. as the lowest he was aware of ; Mr. Parsons suggested 2·5. Only a short time since, *Engineering* published some data upon

Mr. Langdon.

the subject, and I have culled one or two results from its pages. On the Metropolitan Elevated Chicago line the lbs. coal per kilowatt was 1·75; at Boston it varied from 2·61 to 4·13. I have not here the whole of the figures, but if my memory serves me right there were several results given with respect to Boston, and the average came out at a little over 3 lbs. Baltimore City was 3·23 lbs., and Brooklyn City 3·0. Again, the *Engineer* of May 25th, dealing with the Berlin tramways, gives 2·1 as the amount of coal used per kilowatt-hour with super-heated steam, and 2·3 with saturated steam.

My station charges have not been in any way criticised, but the cost of drivers has been mentioned by more than one speaker. Mr. Crompton considers that a less wage might be paid to the class of drivers who would be required to handle the electric locomotives or motors. Possibly that would be so. I have not claimed any exception for that, but, on the contrary, I have in all my charges, as I thought, endeavoured to deal with them in such a liberal manner that they should merit no adverse criticism. There is no doubt that the cost of drivers is a very heavy charge indeed, but I do not see how it is possible to have it to any extent modified unless the traffic arrangements are also modified. With regard to the repair and renewal of machinery, Mr. Crompton very properly pointed out that there would be no boilers practically beyond those required at the generating stations, and that consequently there should be a great reduction in the annual cost, because a great deal of the charges for repairs in connection with the locomotive is in connection with the boilers. He considered—and I think one or two other speakers who followed him also considered—that my charges in connection with the repair and renewal of machinery were too high, and that something less than twopence per train-mile ought to be obtained. In this I quite agree, but I prefer to adhere to the figures I have adopted for the reason just advanced.

Professor Carus-Wilson has been kind enough to send me a copy of his paper on polyphase traction, read before the members of the Institution of Mechanical Engineers. I readily and heartily acknowledge the Professor's kindly courtesy, but I may perhaps be allowed to say that whatever is done with respect to the introduction of electricity for the operation of main-lines of railways it will have to be, I feel convinced, of the most simple possible character. For instance, polyphase working would require two contact rails, and that alone would introduce a great deal of complication.

I am sorry to find that time forbids the completion of my remarks. I will endeavour to complete what I was desirous of saying in writing.

Mr. W. Langdon (*communicated January* 23, 1901): The lateness of the hour, and consequently the few moments which could be allotted me in which to attempt to reply to the observations of the several speakers, obliged my remarks to be so hurried, and so incomplete, that I feel it will be necessary to trespass somewhat largely upon the privilege accorded me to supplement what I then said by this communication.

It is not my intention to review the remarks of each speaker, but to deal with the salient points under their respective heads. These I regard as—

Mr. Langdon.

(1) The Tractive-Effort formula employed by me;
(2) The Coal factor;
(3) The Hypothetical Electrical System employed for comparison.
And to these I propose to add certain observations emanating from myself in relation to—
(4) The Rolling Stock and Permanent-way arrangements.

Tractive-Effort Formula.—The results arrived at under Table IV. depend largely upon the formula which I have employed. If inaccurate, or affording too low a factor, my deductions will call for modification. The question therefore is whether the formula employed is a reasonably

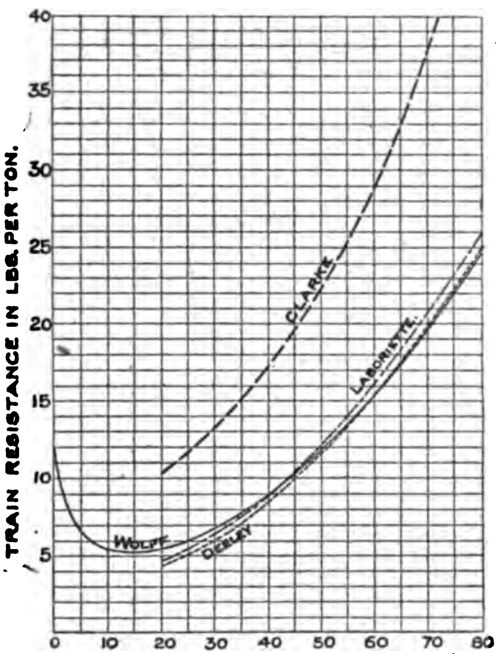


FIG. C.—Extracted, by permission, from Fowler's "Mechanical Engineer" Pocket-Book for 1900.

fair one. By the courtesy of Mr. W. H. Fowler, the author of "Fowler's Mechanical Engineer Pocket Book for 1900," I reproduce the Chart which occurs at page 333 of that work, showing the Tractive Effort curves plotted from results arrived at by various experimenters. Wolff's curve is derived from data published in the *Mechanical Engineer* for February 25, 1899, and is as follows :—

$$R = 3\left\{\frac{V+12}{V+2}\right\} + \frac{V^2}{300};$$

Mr. Langdon.

R being resistance in pounds per ton, and V velocity in miles per hour.

This formula when worked out for a speed of 50 miles per hour provides a lb.-ton factor of 12. That adopted by me provides for 13 lb.

At page 138 Fowler also furnishes the following formula for speeds over 20 miles an hour as that established by "modern experimenters":—

$$R_1 = 3 + \frac{V^2}{290};$$

and where the speed is less than 20 miles an hour—

$$R_1 = 3\left(\frac{V+12}{V+\ 3}\right) + \frac{V^2}{300}.$$

R_1 in each case being resistance in pounds per ton of engine, tender, and train; and V speed in miles per hour.

In each instance these afford a lower lb.-ton factor than that employed by me.

Laboriette's curve is a trifle higher than Wolff's, while Wolff's and Deeley's are practically synonymous.

I here insert a letter which I have received from Mr. Deeley.[1] Mr. Deeley, as is well known, occupies the position of Mr. S. W. Johnson's Chief Technical Officer, and has for many years been associated with investigations of this character.

On the chart, copy of which I have produced, is a curve representing Clark's deductions; and in "Molesworth" is the formula—

$$R = 6 + {\cdot}009\,V^2;$$

where V = Velocity of train, miles per hour, R = Resistance, lbs., per ton of train, which, according to a footnote, is said to be "modified for oil lubrication, from D. K. Clark's formula $\frac{V^2}{171} + 8$ for lubrication by grease." (The name is spelt differently, but the curve is apparently based upon the above formula.)

Working these out for the same speed, viz., 50 miles per hour, it appears that the modified formula—presumably intended to apply to oil lubrication—produces a lb.-tonnage of 28·5, while that assumed to be applicable to grease lubrication affords a result of 22·62 lbs.; indicating that the lubrication by grease is superior to oil lubrication, an opinion which few will, I think, share.

"Molesworth" also gives a formula by Harding, stated to be inapplicable to low speeds. By this formula the table which accompanies it shows that for a speed of 50 miles the lbs. ton is, by experiment, 32·9, and by formula, 35·3.

Summarised, I classify these with their results as under:—

[1] Printed in Report of Meeting of Glasgow Local Section (see p. 200).

Mr. Langdon.

Deeley		All agreeing in practically	Lbs. Ton at speed of 50 miles per hour.
Wolf			
Laboriette			
Barbier	Quoted by Deeley	$3+\frac{V^2}{250}$	13·00
Du Bosquet			
Fowler		$3+\frac{V^2}{290}$ ...	11·62
Do. (for low speeds)...		$3\left(\frac{V+12}{V+3}\right)+\frac{V^2}{300}$ ...	11·84
Clark (modified)... ...		$6+\cdot 009\,V^2$ ...	28·50
Do.		$\frac{V^2}{171}+8$ ...	22·62

It is not clear that Clark's formula embraces the weight of the engine and tender. If so, then the H.P. required for a train of 275 tons, including engine and tender, when travelling at a speed of 50 miles an hour, would be, by formula, $6+\cdot 009\,V^2$, 1045, instead of 477 as shown by me. If it is not intended to embrace the engine and tender, then the H.P. would be 703. In the first case a doubtful attainment on the part of the locomotive, and, in either case, excessive. Indeed the results appear to be such as to favour the impression that the higher formulæ must be the result of experiments conducted under conditions very different to those attending the tests carried out by other experimenters.

As somewhat confirmatory of the result worked out by me it may, perhaps, not be out of place to quote, from Mr. Johnson's Presidential Address already referred to, the following sentence in reference to a train of 256 tons: "One of my single-driving-wheel locomotives, running between Nottingham and London, burns per indicated horse-power per hour, from 2·9 to 3·1 lbs. of coal with ordinary firing, and uses about 29 lbs. of water per horse-power, per hour, when the *mean* indicated horse-power is about 400." The italics are mine. From this I presume it may be inferred that 400 H.P. is the mean indicated power exercised during the journey. The horse-power for such a train and speed is shown in my Table IV. as 477.

Although a copy of my paper was sent to the General Managers and the Locomotive Engineers of all the chief railways, Mr. Pickersgill, of the Great North of Scotland Railway, is the only locomotive engineer who has referred to the subject, and, as I read Mr. Pickersgill's remarks, he regards the formula as fairly correct, but considers certain allowances for curves, gradients, etc., have not been made. The formula is the result of a large number of experiments with ordinary made-up trains, and, so far as I can ascertain, does take into consideration all these points as well as acceleration. As Mr. Pickersgill has, in conformity with others whom I have quoted, made numerous experiments to get at absolute results, and as his results would appear to agree with

Mr. Langdon. those obtained by the majority of other experimenters whom I have quoted, as well as with that formula which I have employed, I can see no reason for any modification of my figures—especially as Mr. Pickersgill's investigations would appear to have extended to goods as well as passenger trains. It should, however, be borne in mind, as I have indicated in the paper, that my deductions are framed on a clear run by the trains at the speeds indicated. Some allowance may be necessary for frequent or exceptional acceleration, but it is not proper to base this upon data obtained from "omnibus" traffic such as that of the City and South London, the Metropolitan, or other subway trains. With such a central generating scheme as I have outlined, very much of the demand for acceleration would be met by the cessation of demand due to trains moving under momentum prior to stoppage.

Coal Factor.—Probably the item next in importance as affecting the accuracy of my conclusions is that of the coal bill. It has been argued that the whole question of economy centres upon the coal consumption. No doubt coal is a large factor, but I do not agree that it dominates the issue. The question is whether, with a continuous output, day and night, with a load factor closely approaching 100 per cent., it is possible to generate one thousand watts of electrical energy, for one hour, with 3 lbs. of coal—coal of an ordinary character, such as is known as Yorkshire or steam coal.

In dealing with this branch of the question at the conclusion of the discussion, I quoted some cases in which electricity power stations had complied with this condition, but being pressed for time I did not give the load factors. I do so now.

	Output.		Coal per Kilowatt-hour.
Metropolitan Elevated Railway, Chicago	2,070,537 kw.	...	1·75 lbs.
Boston (1897):—	Load Factor.		
Albany St. Central	34·8 per cent.	...	2·86 ,,
East Cambridge	52·2 ,,	...	3·24 ,,
Dorchester	33·6 ,,	...	2·48 ,,
East Boston	22·3 ,,	...	3·18 ,,
Charlestown	46·4 ,,	...	2·61 ,,
Allston	47·0 ,,	...	4·13 ,,
		Average	3·083 ,,
Brooklyn City Railroad (1897):—			
Kent Avenue Station	36 ,,	...	3·0 ,,
Southern Station	30 ,,	...	3·5 ,,

The whole of these generating stations have not a continuous output. They are shut down, or the fires are banked for some hours out of the 24; while it will be seen that the load factor is feeble in comparison with that which forms the basis of my calculations. I have already referred to the results obtained by the Berlin tramways power-station, viz., 2·1 to 2·3 lbs. per kw. hour, and it may perhaps not be out of place

if I here quote from a letter received by me from Mr. John Meldrum, of Manchester, who, after discussing the subject in a very full and complete manner, observes: "We should consider that the 2½ lbs. mentioned in your letter could, under the favourable conditions of constant maximum load, such as you name, be reduced in practice to 2¼ lbs. or even slightly less, of course taking the plant to be of the most modern and economical character." I am here glad to have the opportunity of thanking Mr. Meldrum for his courteous reply to my inquiries, and ready permission to make use of his letter. Mr. Langdon.

The fact is there exists no electricity generating station working under such a load factor as is possible—and probably only possible—when dealing with a demand such as would be necessary to meet the requirements of a stretch of from 30 to 50 miles of railway filled with trains both day and night, fed from a central generating plant capable of serving such a section of line by substations. The results I have quoted above do not apply to such a condition. Surely then if attained in America, and on the continent of Europe, under less generous conditions, it ought to be possible to attain it in England.

The mode adopted by me for considering the weight of coal required per kw. is a very definite one. The power derived from the electricity generator can be measured to the greatest nicety. This is not so with the steam engine itself. I freely admit I am unable to follow the figures quoted in relation to the consumption of coal by the locomotive —whether the indicated horse-power, or the per ton-load, forms the basis. I take my figures from the data given in Mr. Johnson's Presidential Address (although other and later figures are no doubt to be found in those technical periodicals which from time to time publish such details), for the reason that they are authoritative. Mr. Johnson shows that the average consumption for coal and coke for 24 years was 50·191 lbs. per train-mile, and this had been an increasing factor from 1884, the quantity for 1896 being 54·19 lbs. Now if the London and Bradford express locomotive previously quoted burns per I.H.P. per hour 2·9 to 3·1 lbs. of coal per horse-power, per hour, when the mean indicated horse-power is about 400, it is clear that such engines travelling at the speed indicated burn about 24 lbs. of coal per train-mile. This means that the locomotives on other trains burn *considerably more* than 54·19 lbs. per train-mile; and with these figures before us it is difficult to follow the argument that steam locomotives, when standing, do not consume coal. To an ordinary observer it seems clear that much of the cost at present incurred for coal and coke is due to the consumption consequent upon trains being kept standing at stations and other points *en route*. When a train is put on one side, for another to pass, it must keep steam, or it would not be ready to proceed when called upon. My paper has not been written for the purpose of decrying the steam locomotive. I share the general opinion that the locomotive is, having regard to the purposes to which it is placed, and the conditions under which it has to work, a marvellous piece of mechanism. It is not, having regard to these conditions, an extravagant power producer, and no such suggestion has been made, but at the same time there can be no question that it is more costly than would be electric traction.

Mr. Langdon.

Electrical System.—The object of my paper was to consider, provided main lines of railway could be worked by electricity, whether such a mode of working would be attended with economy. For this purpose it was necessary to *assume* an electrical service. In this spirit I outlined a plant which I thought suitable for that purpose. Purposely I drew the efficiencies of that plant wide, nor did I attempt to crimp the staff, or other charges in connection with the working arrangements. A certain sum was allotted as the cost of the plant, cables, etc., and if this sum is fairly sufficient for the purpose it is scarcely material to the object in view whether the scheme outlined by me is the best that could be devised or not. I believe it would be found adequate, but it is worth while observing that any increase in the capital outlay would scarcely affect the annual charges beyond those required to cover interest and depreciation. While penning these remarks my attention is drawn to the details of the Albany and Hudson Electric Railroad—a line of some 37 miles, the electrical equipment of which is much the same as that outlined in my paper; the potential difference of the primary current being, however, 12,000 instead of 10,000. The service current is the same, viz., 600 volts. The contact conductor rail is outside the traffic rails, and is protected by timbers on either side of it, so that accidental short circuits or shocks to workmen may be avoided. The New York Manhattan Elevated Railroad is also adopting the outside position for the contact rail.

My paper was read, not for the purpose of advertisement, or to serve any personal interest whatever, but that the subject with which it dealt might be discussed, and, if shown to be in any portion erroneous, that the errors might be discovered. Having no precedent much has had to be assumed, and hence my desire that the charges applicable to electricity should not be too narrow, or that I should take into account every source of saving which would attend its use. Perhaps I am wrong in assuming that where my deductions have been questioned it would have been but fair that those who questioned them should have furnished what they believed would prove more accurate. Whether, in the event of electricity proving of service for railway work the plant I have so roughly sketched out for comparison purposes is suitable or not, there can be little doubt that neither it nor any other scheme would be accepted without very careful consideration on the part of the company about to make use of it. My effort has been to show that when the time arrives for such an investigation it may, from an economical and national point of view, be undertaken with every prospect of advantage.

The remarks of Mr. Mark Robinson with regard to the power of the steam unit to be used, and his observations on fast *versus* slow speed engines, will be read, I am sure, with much interest. His observations on the propriety of working the section of line by five local generating stations, rather than by five substations served from one central generating station, merit every consideration, although, personally, I regard the scheme suggested in the paper as in many respects more advantageous. My verbal remarks on this point were so hurried, that I feel I failed to do justice to the economical advantages which should, it appears to me, attend a central and substation scheme.

These lie almost entirely in its flexibility. A variable load in the sub-sections affects very little, if at all, the output from the generating station. The generating station has to meet the *whole* demand, and if this demand is centred all in one sub-section, or distributed equally throughout the whole of the sections, it is, as a load-factor, immaterial. In the case I take, that load is the 14 trains per hour. The load being, so far as the generating station is concerned, fairly constant, there is no waste in coal, water, or labour. Local generating stations would effect a saving in high-tension cables and in transformers, but the equipment of each station would be more costly; for each station must have at least a spare set of generators, and unless each 10-mile section so served were filled with trains, so as to establish a constant demand, the result would be a much lower load-factor. The capital outlay would probably be less, and the electrical efficiency would be higher. A number of short sections thus locally served would possibly afford greater security against failure of machinery, but it would be at the cost of economy. All these are points which will necessarily demand attention when an absolute proposition is ripe for consideration. The governing factor should, to my mind, be the *load-factor*—such a section of line as would insure a constant output. Mr. Langdon.

Attention has been directed to the advantages of polyphase systems. A polyphase system would appear to be objectionable. In the first place, because it would entail the use of two conductor or contact rails which, at junctions and crossings, would prove extremely difficult, if not quite inadmissible; and in the second place, on account of its speed-rigidity—its inability to admit of acceleration of speed. Railway companies have for years been gradually increasing the speed of their trains, and any electrical system that may come into use must be such as will admit of this acceleration if called for, and also afford as large a diversity of speed in the movement of trains as is at present enjoyed. A mixed traffic has to be dealt with, and this cannot be disregarded. A passenger train differs from goods and mineral trains in that each vehicle is supplied with brake-power which enables the train to be stopped, if necessary, practically in its own length; the brake-power of goods and mineral trains is, however, limited, and they can only travel at a speed regulated by the command which the brake can exercise. Climatic conditions largely affect the speed and operations of trains on overland railways. In dense fogs all traffic becomes congested, and the movement and speed of the trains has to be subordinated to the conditions which prevail. A predetermined speed, which must be rigidly observed, would, I fear, entail difficulties.

Rolling Stock and Permanent-way Equipment.—It was with some surprise I heard Mr. Sprague comment upon the difficulty of producing an electric locomotive competent to deal with such loads as are indicated under Table IV. The more so as I had in my mind an electric locomotive built by the General Electric Company for the Baltimore and Ohio Railway Company, for the purpose of dealing with heavy traffic. I believe I am correct in stating the details of this locomotive to have been announced as follows:—Weight on driving wheels, 96 tons. Draw-bar pull, 42,000 lbs.; starting ditto, 60,000 lbs.

Mr. Langdon. Diameter of driving wheels, 62 in. Length overall, 35 ft. Propelling power 4 motors, each rated 360 H.P.

It is quite true that large electric locomotives have not been built to travel at speeds of 50 miles or more, but I believe this to be simply because there has not been a demand for them. Electricity where employed for railways has not been used in what may perhaps be termed bulk; it has been confined to comparatively light trains—the largest and heaviest locomotives employed in England being those of the Central London. And in all these it is probable that the use of a heavy locomotive has, for obvious reasons, been rather shunned than encouraged. One would imagine that no difficulty in ventilation, construction, or speed would attend the production of whatever is required so long as it does not exceed in space the structure gauge of the railway upon which it is required to travel, and as it does not exceed the safety impact allowed for the structures over which it has to travel. Main overland railways have a permanent way far less liable to the effects of vibration than underground lines.

Some stress has been placed upon the propriety of applying the motors to the carriages instead of employing an electric locomotive. Main-line traffic, embracing goods and mineral trains, can only be worked by the latter.

It is, as I intimated in the concluding portion of the paper, in the arrangements to be adopted for the distribution and collection of the current that the greatest difficulties will be encountered. No doubt, had time admitted of the continuation of the discussion, reference would have been made to this branch of the subject. As it is, the matter has passed unobserved, and yet in it, so far as can be seen at the present moment, there is cause for the greatest consideration. Hitherto, where electric energy has been the power employed, it has been conveyed by overhead conductors, or where a contact rail has been used, it has been in those cases only where the line of railway is not open to trespassers. With overland railways the intrusion of irresponsible people can no more be insured against than rain, snow, or flood. With all large railways an overhead conductor, or a structure for the purpose, is, it is to be feared, impossible. If the current has to be collected from a conductor on the ground, it appears that the conductor must be arranged practically on the same level as the railway metals, or very little above them. It must be protected in some manner to avoid accidental, and, if possible, intentional short-circuits. Assuming that this may be accomplished, its position in relation to the railway metals next demands attention. If laid on the sleepers which carry the railway metals, difficulties will probably arise in executing repairs to the running roads. It would therefore appear desirable that it should be entirely dissociated from them. This again will necessitate the use of a flexible collector, for if the contact rail is dissociated from the sleepers which support the railway metals, it will be necessary to ensure for it a position relative thereto.

Although it has been, I believe, fully demonstrated that the current can be collected from such a contact rail at a speed of 60 to 70 miles an hour, further experiments dealing with maximum quantities of current might with advantage be undertaken.

Sections of line subject to floods could only be dealt with by raising the line above the flood level—probably not a very costly matter, and, moreover, a proceeding desirable from other points of view. Mr. Langdon.

Conclusion.—In concluding these remarks I feel it desirable that I should emphasise the fact that the purpose of this paper has been to show that, *primâ facie,* there are grounds for assuming that, when those impediments which now exist have been overcome, the contemplated change will not be attended with pecuniary loss, but that on the contrary, it may, and probably will, be accompanied by many advantages. As a step—the first step—towards this it has been necessary to compare the cost of the possible new agent with that of the present. In doing so I have drawn *two* comparisons with the cost incurred under the steam locomotive. One with the *average* yearly cost for a period of 24 years; the other with that for 1899. It is evident that in criticising my deductions the fact that my comparison was not confined to the cost for the "average" period has been overlooked. Those deductions show the assumed cost of electricity at 7·021; steam, for the average of 24 years, at 8·943, and for 1899, at 10·218 pence. Now assuming, for the purpose of argument, that the formula which has been so much discussed, but which has at last been practically confirmed, is not sufficiently high so far as it applies to goods and mineral trains, it can but affect the electrical charges to a very slight extent; it cannot, for instance, double them. But suppose the generating charges *are* doubled, there will still, in comparison with the "average" yearly cost of steam, remain a balance in favour of electricity; and of practically a penny-halfpenny per mile (or some £271,583), on the 1899 comparison, *i.e.*, without deductions for contingencies! Add to this, as has more than once been commented upon by those who have participated in the discussion, the fact that the efficiencies upon which the output of electrical power is based are low; that all trains have been regarded as loaded trains; that more than the hourly average number of trains are embraced by my calculations; that the cost of repairs and renewals, as set forth by me, are deemed excessive; and add to this many other obvious savings such as the painting of stations, the redecoration of coaches, the saving in the abolishment of isolated plants, and I doubt if there will be many who will condemn me for having overstated the case for electricity.

The position I have assumed for electricity is necessarily somewhat hypothetical. The question is, Is it reasonably fair? It has been pointed out that the electrical service has been framed to meet only the average hourly train service as ascertained by Tables II. and III., and that occasions must arise when this will be exceeded. This will undoubtedly be so,[1] but my reason for selecting the section of line indicated was that it was a busy line—a section of line that might be

[1] Since the reading of this paper I have obtained from the stations named a return of the greatest number of trains passing those points on any one day. Here are the figures:—

Luton, January 25th ... ... 297 = 12·4 per hour.
Harpenden, August 4th ... ... 331 = 13·8 per hour.

The basis taken in the paper was 14 per hour.

Mr. Langdon.

truly regarded as a full line, and that it might, consequently, be looked upon as worked up to its full capacity. Obviously, however, if there were more trains—provided electricity is, *per train-mile*, less costly than steam—the greater the number of trains and consequently the greater the mileage, the greater would be the saving. It might, and probably would, mean an increased capital outlay—an increase in interest charge and repair—to be met by the additional saving on the additional mileage run.

One or two members have, in their observations, expressed some difficulty in following me in regard to the number of trains per hour which I have accepted. My calculations are based upon the kilowatt-hour for the power to be generated; upon the power shown to be required for dealing with one hour's traffic; and, for the purpose of comparing electric with the steam charges, with the train-mileage accomplished in the hour. The number of trains passing any one point on the length under observation in the twenty-four hours may surely be taken as the number for the day. I have taken two points. The number of trains was 287 in one, 297 in the other. The greater of these divided by 24 will give an average of 12·38 per hour. I have taken 14 trains, which 14 trains, as I show, should travel 479 miles in the hour. I am afraid I cannot put it clearer. I did try to collect and tabulate the times of trains passing several points, but it became too complicated to be dealt with in such a paper. Perhaps it would render my reasoning clearer if my critics in this instance would assume either of the returns as applicable to one end of the section—say Bedford. Thus for the twenty-four hours there would be 297 trains in all arriving or departing, and $\frac{1}{24}$th of this number would be the average hourly number of trains for the period.

It has been stated that the mere cost of shunting-work would more than absorb the £50,000 set apart for contingencies. Surely those who advance these arguments lose sight of the fact that much of the cost for shunting lies in the time the engine is doing no work. Should it ever transpire that the work of shunting-yards can be worked electrically, a great saving in this respect should attend it.

It is primarily to main-line working that I have addressed myself. Several speakers have intimated that electrical traction will first be employed on the branch lines. There are, no doubt, branch lines the locality of which would largely benefit by more frequent train facilities; but we have to bear in mind that the majority of branch lines have been established to secure connection with the main line, and, consequently, that the traffic thereon must be largely dependent upon the main-line service. If electricity can be employed for branch lines, why not for main lines? Presumably the only reason is the large power demand required for the latter. The electrical system employed must be identical—that which serves the one must also serve the other. Branch lines will be most economically served as a part of a main-line installation; that is manifest. Those difficulties that attend main-line working will also, although in a less degree, attach to branch-line working, and as it is in the operation of the former that the greatest advantages and economies will arise, it is towards its accomplishment that efforts should

Mr. Langdon.

be directed. Apparently what is required is a safe means of conveying the current to the trains, and an electric motor that shall perform the functions now dispensed by the steam locomotive. The crux of the whole question would seem to reside in the mode to be adopted for the distribution of the current. Any difficulty which will attend the determination of the site of the conductor rail, or the means for collecting the current, is not likely to prove so serious as that which will accompany the provision against vicious interference with the former for the purpose of disorganising the traffic. The subject is one which we may be sure will claim the attention of inventors. Legislation may aid, but all the powers of the law will not prove so efficacious as a means by which it shall not be possible, or, at least, by which it shall be so difficult to tamper with it as to render it not worth the risk. To protect the rail from accidental interference presents no difficulty. That may not only be done, but in the doing of it insulation may be aided. It is the malevolent action of the miscreant, and of the mischievous, which has to be guarded against.

Mr. Alexander Siemens, in directing attention to the title of the paper, has asked how the trains hauled by the steam locomotive are to be dealt with at that point from which the traffic has to be conducted by the electric locomotive.

My paper aims at the supersession of the steam locomotive—at its replacement by the electric locomotive. No railway company would break up their time bill, or dislocate their service in the direction of Mr. Siemens' question for the purpose of introducing electricity. The electric locomotive, to compete with the steam locomotive, must be able to couple on to a train and haul it away precisely as is now done by the steam locomotive. Section by section of a railway system might then become converted to the application of electrical agency without undue sacrifice of material, and with absolutely no inconvenience to the traffic.

It has been said that economy will not prove the means by which railway companies will be induced to seek the aid of electricity; that opposition—the establishment of competing lines—will prove the powerful factor. Whether it is opposition or not, economy must be the agent which will chiefly operate. Competition, as perhaps may be the case between Manchester and Liverpool ere long, means loss of traffic, and loss of traffic, loss of revenue, and few commercial institutions can prove indifferent to that. Electricity will no doubt effect many changes other than those it is apparently destined to effect in relation to our large railway systems.

Very heartily do I tender most grateful thanks to all those who have taken part in the discussion, or otherwise contributed towards the elucidation of the question dealt with by me. Although I think I may fairly claim to have substantiated the ground assumed in the paper, I welcome such criticism as has been of an adverse character. I feel that it is well that every possible view should be advanced in order that those who may desire to study the question may lose sight of no point which may be material to a fair issue.

The Three Hundred and Fifty-Fourth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 13th, 1900, Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on December 6th were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that they should be suspended.

The following transfers were announced as having been approved by the Council, viz. :—

From the class of Associates to that of Members—

Harold William Couzens.

From the class of Members, Northern Society of Electrical Engineers, to that of Members, Institution of Electrical Engineers—

Llewellyn Andrew.

A donation to the Benevolent Fund was announced as having been received since the last meeting from Mr. Augustus Stroh, to whom a vote of thanks was unanimously accorded.

ON RAPID VARIATIONS IN THE CURRENT THROUGH THE DIRECT-CURRENT ARC.

By W. DUDDELL, Wh. Sc., Associate.

It may be thought by some that the title of this paper is rather contradictory in that there should not be any variation in the current through a direct-current arc. I will therefore explain at once that I simply use the term "direct current" as implying that the current is supplied by cells or by a direct-current dynamo, and not as implying that the current is necessarily constant in value. It may also be as well to state that by *Arc* I do *not* mean *Arc Lamp*, as all the effects to be described are quite apart from those produced by regulating mechanisms.

The effect of varying the current through the direct-current arc very slowly, so slowly in fact that the carbons have time to burn into shape corresponding to each value of the current, has been investigated by many experimenters, but it is to Mrs. Ayrton[1] that the honour belongs of giving a complete investigation of all that occurs when any of the variables in the direct-current arc are changed in any way. The other extreme, namely, very sudden changes in the current, has also been investigated by Mrs. Ayrton,[2] thus leaving a gap in the experimental evidence as to what occurs between very slow variations and isolated sudden changes in the current.

The present paper is an attempt partly to fill this gap by giving an account of what occurs when the current is periodically varied more or less rapidly over a range which is very small compared with the mean value of the direct current.

The current through a direct-current arc supplied with power from any circuit may vary either owing to changes taking place in the circuit, such as variations in E.M.F. or resistance, or owing to effects in the arc itself, such as hissing, humming. Although any variation in the current naturally entails a corresponding change in the arc itself, it will, I think, be found convenient to classify the observed effects according to whether the primary cause of the variation is in the arc or in the circuit which supplies it.

PART I.

CAUSE OF THE VARIATION OF THE CURRENT IN THE CIRCUIT SUPPLYING THE ARC.

The effects of varying the current may be divided under four heads, viz., the effect on the P.D. between the terminals of the arc, on the light emitted, on the shape of the craters, and on the vapour column. These will be considered in order. I shall assume in all cases in Part I. that the amplitude of variation of the current from the mean is small, generally much less than 10 per cent., and that the arc experimented on is neither hissing nor humming.

[1] *The Electrician*, vols. xxxiv., xxxv., and xxxvi.
[2] *The Electrician*, 1895, vol. xxxiv., pp. 471, 541.

EFFECT ON THE POTENTIAL DIFFERENCE PRODUCED BY VARIATIONS OF THE CURRENT.

If the current varies very slowly, then the relation between the P.D. current and length is that given by Mrs. Ayrton's curves. Directly the rate of variation is increased so that the carbons have not time to burn into shape, corresponding with the instantaneous values of the current, the relation will be changed, and it is conceivable that *if* the rate of variation were high enough and the amplitude small enough, the conditions of the arc would in no way be changed, so that the ratio of the change in P.D. to the corresponding change in current, would be a constant and equal to the true resistance of the arc. I shall show later that this assumption, which is the basis of several experiments on the resistance of the arc, notably those by Messrs. Frith and Rodgers,[1] requires a much higher rate of variation of current than they employed.

One of Mrs. Ayrton's curves, contained in a letter by Prof. Ayrton to *The Electrician*,[2] illustrates very well how the connection between the P.D. and current depends on the rate of variation of the latter. This curve shows that the *first* effect of *suddenly increasing the current* through a cored-solid arc [3] is to cause a *transient rise in the P.D.* between the terminals; the effect of a *slow increase of current* being, as is well known, to produce a *decrease in the P.D.* This first transient rise in the P.D. which was obtained with a cored-solid arc, was also, I believe, obtained with a cored arc, but I am unaware of its having been observed for a solid arc.

Thinking that this might be due, as pointed out by Prof. Ayrton, to the extreme quickness of the phenomenon when *both* carbons were solid, I tried to record the transient rise in P.D. for the solid arc by means of an oscillograph, the sudden increase of the current being obtained by discharging a condenser through the arc. This experiment was successful, and a transient rise in P.D. was observed, *the P.D. and current increasing together, but only for about*

[1] *Proceedings of the Physical Society*, 1896, vol. xiv., p. 307.

[2] *The Electrician*, 1896, vol. xxxvii., p. 321.

[3] "Solid," "solid-cored" and "cored" arc mean, respectively, arc between two solid carbons, between one solid and cored, and between two cored carbons; the top or + electrode being always placed first.

$\frac{1}{5000}$ *second.* At the end of this very short time the P.D. decreased with an increase of current in the ordinary way.

If it can be assumed that during this first $\frac{1}{5000}$ second the conditions of the arc are not changed, then the solid arc has a positive resistance, contrary to the results obtained by Messrs. Frith and Rodgers, and it is at any rate evident that, had the frequency of their superimposed alternating current been 5,000 $\sim$ per sec. instead of 250 $\sim$ per sec., the sign of the resistance as obtained by them would have changed, though I do not say that even at that frequency its true value would have been obtained. In any method for measuring the resistance of the solid arc which depends on the change in the P.D. produced by a change of current, these changes must, therefore, take place in less than $\frac{1}{5000}$ sec. in order not to allow the arc conditions to change; results to be described later indicate a still shorter time.

I will not, however, pursue this subject any further, as it would unduly extend the length of this paper to include a description of a complete series of experiments on the resistance of the arc which I have recently completed.

Effect on the Light Emitted produced by Variations of the Current.

It is well known that the light of the arc varies when the current is changed, though how small and rapid the variation in current may be and yet produce a perceptible change in the light does not seem to have been investigated. Professor Fleming and Mr. Petavel[1] and Mr. Burnie[2] have determined the instantaneous values of the light and current in the case of alternate-current arcs, and have found that the variation in light roughly follows the variation of the current; the maximum luminous intensity occurring about $\frac{1}{1000}$ sec. later than the maximum current. Herr Görges[3] has also noticed that the variations in the current due to the teeth on the armature of a dynamo produced an appreciable variation in the light at the rate of 300 per second.

[1] *Proceedings of the Physical Society*, 1896, vol. xiv., p. 115.
[2] *The Electrician*, 1897, vol. xxxix., p. 849.
[3] *Electrotechnische Zeitschrift*, 1895, vol. xvi., p. 548.

In order to test how rapid and how small a variation of the current from the mean could be detected in the light of the direct-current arc, I arranged an arc so that its image as seen through a central slit parallel to the carbons was projected on to a rapidly falling photographic plate, the instantaneous value of the current being recorded simultaneously on the same plate by means of an oscillograph. The small quick variations of the current through the arc were produced by passing the oscillatory discharge of a condenser in series with a self-induction through it, so that the arc current consisted of a large constant part on which was superimposed a small ripple which died away after a few oscillations.

By this method I find that in an 8-ampere solid arc a *distinct variation is produced in the light emitted by both the + crater and the vapour column when the amplitude of the variation of the current from the mean is only* 3 *per cent. and the frequency of these superimposed variations is as large as* 4,300 ∿ *per sec.* At this frequency the variation in light became indistinguishable when the amplitude of the variation of the direct current was reduced to 2 per cent.

Owing to the difficulty in estimating the points of maximum density in the band on the plate which represents the light emitted in consequence of the smallness of the variation of the current and therefore of the light, I was unable to be certain whether the maximum light lags behind the maximum current; but if it does, the lag is very slight, not exceeding $\frac{1}{10000}$ sec. for an 8-ampere solid arc.

It must be remembered that the above variations of light are those of the actinic rays which affect the photographic plate; the visual rays will probably vary in a similar manner, though possibly not to the same extent.

Effect on the Craters produced by Variations of the Current.

Mrs. Ayrton tells me that she noticed that the variations in the current used by Messrs. Frith and Rodgers, who superimposed an alternating current of 0·5 to 1·0 ampere R.M.S. value, at frequency of 100 ∿ per sec. on a 10-ampere direct-current arc, so altered the shape of the ends

of the carbons that she could easily distinguish them from normal carbons formed without any variation in the current. I find that if the superimposed alternating current be reduced to 0·1 ampere under the same conditions, the ends of the carbons appear unaffected.

EFFECT ON THE VAPOUR COLUMN PRODUCED BY VARIATIONS OF THE CURRENT.

SOUNDS.

Corresponding with each value of the current through the arc there is probably a definite cross-section of the vapour column, so that if the current varies rapidly through an arc of fixed length, the volume of the vapour will also vary and sound-waves will be given out. This, I believe, is the generally accepted explanation of the humming of the alternate-current arc.

In the case of the direct-current arc, sounds are also emitted even when the variations in the current are very slight. For example, the variation of current caused by the commutator segments of a direct-current dynamo passing under the brushes can be heard in the arc. This variation of the current caused by the commutator segments, even when in good condition, was found by Messrs. Frith and Rodgers [1] in the case of a 5 k.w. two-pole machine to vary between 2.5 and 9 per cent. of the mean current according to the position of the brushes.

Another striking example of how sensitive the arc is to small variations in the current is furnished by the fact that a Wehnelt interrupter, working an induction coil on the direct-current street mains, will cause any arc supplied by the same mains to give out the same noise as the interrupter itself, even when a considerable distance intervenes between the place where the arc is connected with the mains, and where the interrupter and coil are joined on, as observed by Herr Simon,[2] Mrs. Ayrton, and Mr. Jervis Smith.[3]

It must be clearly understood that the arcs here referred to are normal silent arcs; that is, if they were supplied with

[1] *Proceedings of the Physical Society*, 1896, vol. xiv., p. 307.
[2] *Annalen der Physik und der Chemie*, 1898, vol. lxiv., p. 233.
[3] *The Electrician*, 1899, vol. xliv., p. 16.

a really steady current they would have been practically silent.[1]

In order to determine what variation in the current was necessary to cause the arc to emit a clearly audible note, the current from a high-frequency alternator, kindly lent by Sir D. Salomons, was superimposed on the direct current by the method shown in Fig. 1. The current from the alternator passes through a condenser F, a dynamometer D, and the arc in series; and it is practically prevented from flowing through the cells which supply the arc by the self-induction L. The direct current is prevented from flowing through the alternator by the condenser F.

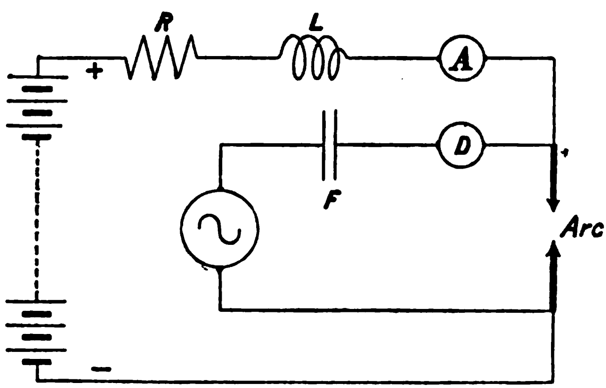


FIG. 1.

It was found by this means that a 10-ampere direct current, solid or cored arc, length 3 to 5 mm., would produce a distinct note even if as small a R.M.S. current as $\frac{1}{1000}$ ampere, as measured by D, was superimposed on the direct current for frequencies of the added current from a few hundred up to 8,000 ∿ per second. Thus *a variation of the order of* 1 *part in* 10,000 *from the mean current will alter the vapour column sufficiently to produce sound-waves.*

Further experiments with another alternator and R.M.S. superimposed currents of $\frac{1}{20}$ to $\frac{1}{10}$ ampere on a 10-ampere solid arc, proved that the sounds only became inaudible at frequencies approaching 30,000 ∿ per second.

[1] Absolute silence is almost impossible, as the least want of homogeneity, or impurity in the electrodes, causes small spits and sounds.

At these frequencies I am uncertain whether the arc had really ceased to give a note, as the ear fails to detect sounds of so high a pitch.

This sensibility of the arc for very small changes in its current explains the fact that not only can rapid variations of current in any circuit supplied from the same generator as the arc be heard in the arc, but also variations of current which occur in a totally independent circuit supplied by a separate generator can be detected in the arc due to mutual induction between the two circuits.

Arc as a Telephone Receiver.

The fact that the arc is sensitive to such small variations

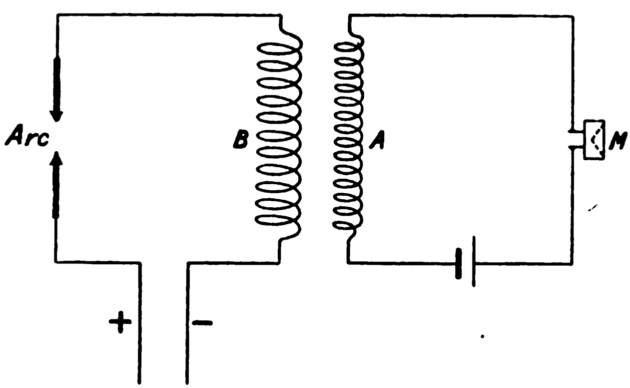


Fig. 2.

in the current and over such a wide range of frequency, at once suggests that the direct-current arc might be used as a telephone receiver. This suggestion, which was made in a leader of *The Electrician* in 1899, had already been carried out by H. Simon [1] in 1898.

The method used by H. Simon for superimposing a microphone current on the main arc current is shown in Fig. 2, in which A and B are two coils having mutual induction, and M the microphone. The current through A varies when M is spoken into and induces E.M.F.'s in B, which vary the current through the arc in such a way that it reproduces sounds and even speech distinctly.

[1] *Annalen der Physik und der Chemie*, 1898, vol. lxiv., p. 233.

The variation of the current through the arc obtained by this method is not as large as it might be, as the E.M.F.'s induced in B have to send currents round the whole arc circuit, including any steadying resistances, and also through the self-induction in the armature, if a dynamo is used, instead of only through the arc where the varying currents are actually required. I have obtained a better result by replacing the alternator of Fig. 1 with a microphone and mutual induction as shown in Fig. 3. A and B are the two coils of a mutual induction, F a condenser of about two or three microfarads, and L a high self-induction, the object of the self-induction being to prevent the microphone currents flowing round the cells instead of through the arc.

With this arrangement and suitable arc conditions, to be explained later, *the arc will speak sufficiently loudly and clearly to be heard at a distance of* 10 *to* 12 *feet in a quiet room.* [Experiment.][1] The sound-waves given out by the arc are, therefore, of such an intensity that when the energy is spread over a spherical surface of 20 feet diameter, the ear placed at any point can hear speech distinctly. It seems probable that if all the energy available could be collected and concentrated on the ear, very powerful sound-sensations might be produced.

The loudness of the sounds given out by the arc is increased by lengthening the arc, as this increases the volume of the vapour column which emits the sounds. It would also seem as if increasing the main current which increases the cross-section of the arc should also be beneficial, but experimentally I have not found

[1] NOTE (*added February 1st*, 1901).—As I have had several inquiries from experimenters wishing to repeat this experiment, I append some data of the apparatus actually used at the meeting.

The microphone M was supplied by the National Telephone Company and intended for long-distance transmission, two accumulators being used in series with it. The mutual induction A : B consisted of a solenoid 30 cms. long wound with about 1,200 turns of No. 18 D.S.C. wire in six sections having an ron wire core about 15 mm. diameter. Diameter of solenoid over winding 54 mm. For the experiments 3 sections = 600 turns were used for A, and o sections = 400 turns for B.

Resistance of A = 1·52, of B = 1·53 ohms. Mutual induction 25·3 × 10^{-3} nrys. Cored carbons were used in the arc, the other data being those given ove.

If a suitable mutual induction A : B is not available a self-induction may be d, by connecting the leads from the microphone and cells to the terminals instead of those of A, B being now simply a coil having high self-induc- and low resistance.

any appreciable gain. The best results have generally been obtained with a current of 10 to 12 amperes, carbons 11 to 13 mm., and an arc length of 20 to 30 mm.

To obtain these long lengths with ease, it is necessary to use cored carbons or some other means of introducing foreign bodies, such as salts of potassium and sodium, into the arc, for there is not much doubt that the stability of the arc between ordinary cored carbons is due to the presence of potassium silicate in the core.[1] (See also Appendix I.) These salts may be introduced either by soaking the carbons in their solutions, or by using them as cores. Mr. Jervis

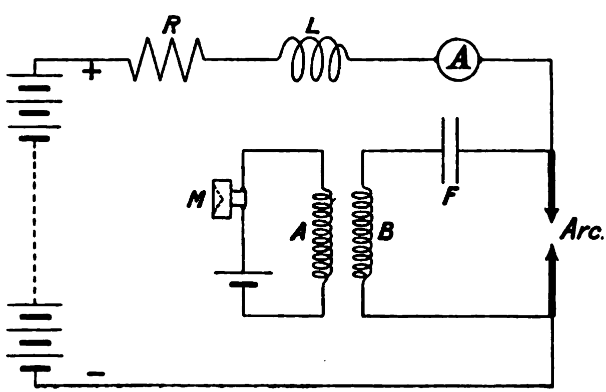


FIG. 3.

Smith has recommended the insulator glass as a core, which I find works well.

ARC AS A TELEPHONE TRANSMITTER.

Before leaving the subject of the use of the arc as a telephone, it will be convenient to consider its use as a telephone *transmitter*, though this subject strictly belongs to Part II. of this paper.

H. Simon found that if he replaced his microphone in Fig. 2 by a telephone receiver, *any sounds made near the arc were heard in the receiver.* In this case, as before, I find

[1] See Duddell and Marchant, *Proceedings of the Institution of Electrical Engineers*, 1899, vol. xxviii., p. 66; Blondel, *International Congress of Electricity*, Paris, 1900.

it preferable to modify his method by connecting the receiver in series with a condenser between the terminals of the arc, as in Fig. 4.

A sound-wave striking the arc may affect it in two ways, either by vibrating the arc as a whole and varying its length, or the waves of condensation and rarefaction may alter the cross-section of the arc: both of these effects will tend to alter the apparent resistance of the arc, and hence vary the current through it.

The sounds obtained in the telephone receiver when using the direct-current arc as a transmitter are not generally very satisfactory, as, besides not being very loud, they are obscured by the extraneous sounds due to the small spits and hisses which occur in the arc each time the air gets to

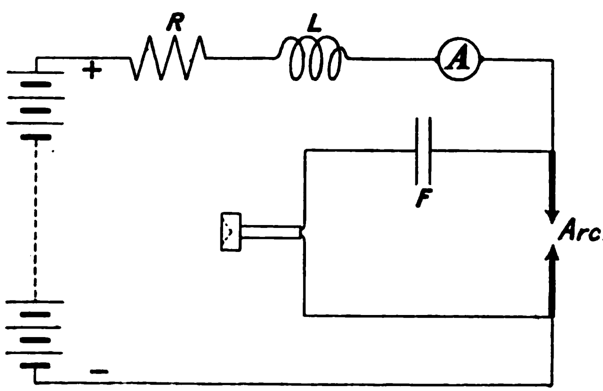


FIG. 4.

the + crater due to any slight defect in the carbons. If a common pair of carbons be used containing cracks and impurities, the noise in the receiver is sometimes unbearable, although there is no outside source of disturbance of the current through the circuit.

In all experiments on the arc as a telephone transmitter or receiver, it is essential that the current generator should be free from rapid variations, or extraneous sounds will be produced. If a dynamo has to be used, then the variations of the current produced by the commutator segments may be minimised by inserting a large self-induction in series with the arc, as in Figs. 1, 3, and 4. This self-induction ·es the double purpose of keeping extraneous variations

Plate I

HUMMING ARC.

CARBONS : + 11 and − 9 mm. Solid "Apostle." Mean P.D. = 50·5 volts. Mean Current = 15·2 amperes.

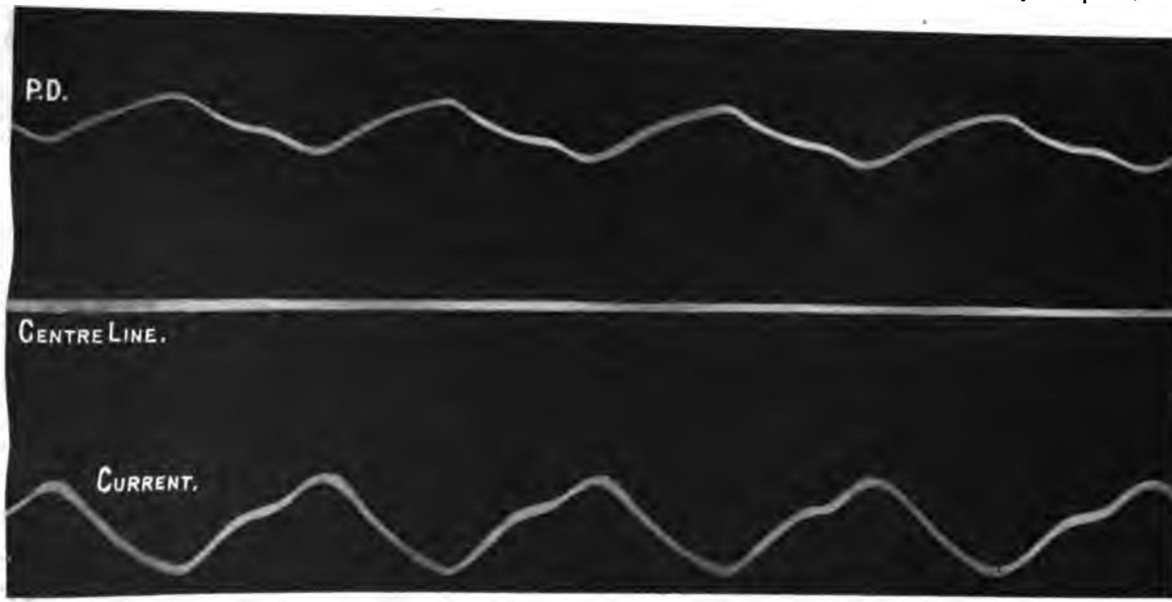


FIG. 5.

Scales :—1 min. = 0·5 volt = 0·186 ampere = $\frac{1}{6400}$ second. Centre Line = 40 volts = 20 amperes.

Plate II

HISSING ARC.

CARBONS: + 11 and − 9 mm. Solid "Apostle." Mean P.D. = 38 volts. Mean Current = 22·3 amperes.

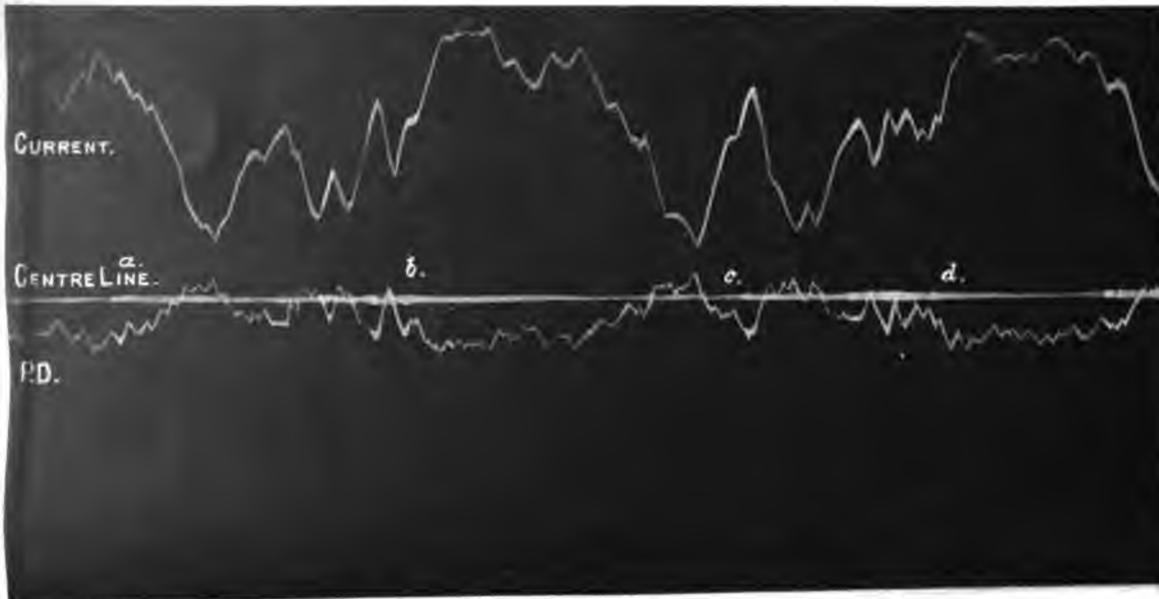


FIG. 6.

Scales :—1 mm. = 0·5 volt = 0·1 ampere = $\frac{1}{8400}$ second. Centre Line = 40 volts = 20 amperes.

Plate III

HISSING ARC.

CARBONS: + 11 and − 9 mm. Solid "Apostle." Mean P.D. = 45.5 volts. Mean Current = 19·5 amperes.

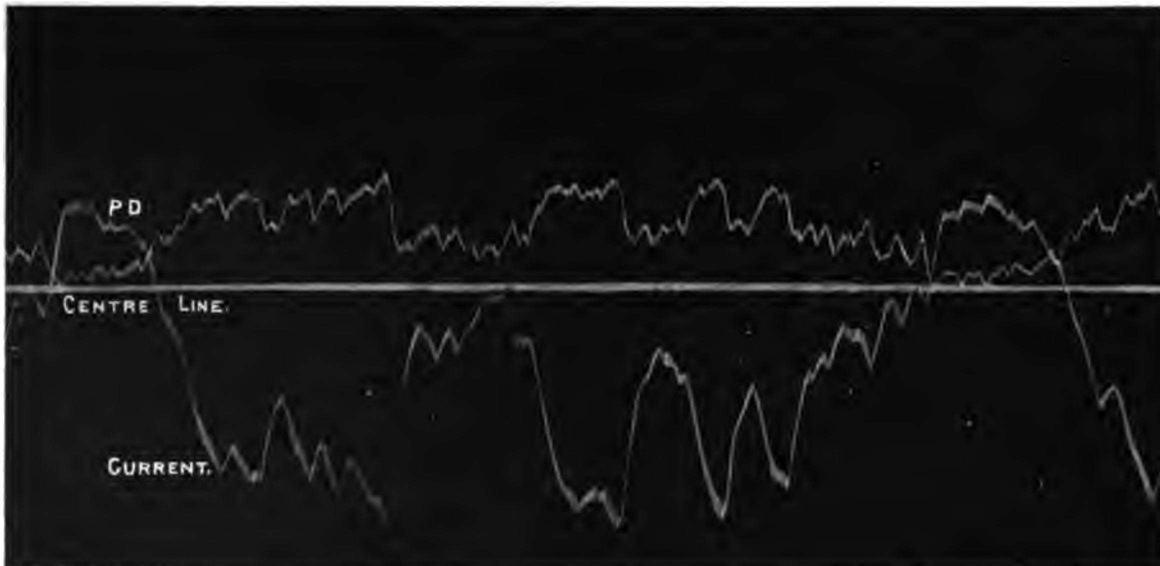


FIG. 7.

Scales :—1 mm. = 0·5 volt = 0·1 ampere = $\frac{1''}{8400}$ second. Centre Line = 40 volts = 20 amperes.

Plate IV A

VERY SHORT HISSING ARC.

CARBONS : + 11 mm. Cored "Apostle" ; – 9 mm. Solid "Apostle."

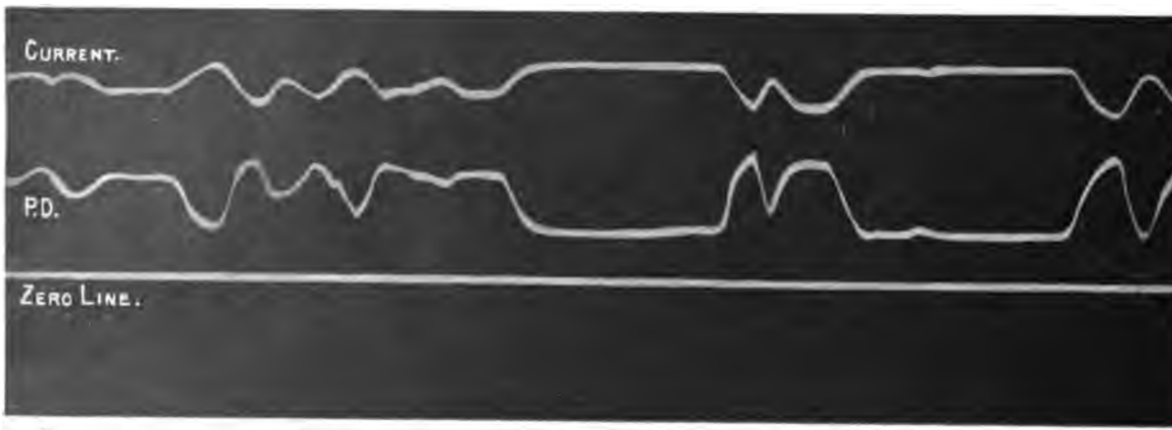


FIG. 8.

Scales :—1 mm. = 1·5 volt = 1·0 ampere = $\frac{1}{8400}$ second.

of the current out of the arc, and of preventing the variations we desire to observe from being dissipated in the source of supply.

Thus we see that *the direct-current arc is not only extremely sensitive to small variations in its current of almost any frequency, but also that it is affected by such small changes of outside conditions as sound-waves produce.* Whether this sensibility can be turned to useful account in telegraphy or telephony remains for future experiment to decide.

PART II.

CURRENT CAUSED TO VARY BY THE ARC.

HUMMING.

Mr. Trotter[1] discovered that the direct-current humming arc rotates, including a coma-like appearance at the + crater, and he also found that the current through the arc varied periodically, the frequency of these variations being the same as the pitch of the humming sound produced, and as the speed of rotation of the arc.

In order further to investigate the connection between the variation of the light P.D. and the current, I have recorded the P.D. and current by means of an oscillograph, the humming arc experimented on being used as the source of light to illuminate the oscillograph mirrors. The arc was so inclined that only the light from the + crater and a small part of the vapour column reached the mirrors. So that the density at any point of the lines represents the photographic intensity of the light emitted at that instant in the direction of the mirrors by the + crater and part of the vapour column, and the distance of the point from this zero line measures the P.D. or the current as the case may be.[2]

A typical example of the variations observed in the humming arc is given in Fig. 5, from which it will be seen that the P.D. current and light emitted in a fixed direction vary in a regular periodic manner with the same frequency.

The variation of the current, which is about 6 per cent.

[1] *The Electrician*, 1894, vol. xxxiii., p. 298.

[2] In Figs. 5, 6, and 7, the centre line is not the zero line, but represents 20 amperes and 40 volts.

from the mean, is not sufficient to account for the large variation in the light emitted in the direction of the mirrors. This periodic variation of the light is most probably due to the fact that the arc rotates so that the + crater alternately either supplies light to the oscillograph mirrors, or is prevented from doing so by being on the other side of the + carbon. The periodic time of the variations of the light will, of course, be unaffected by a change in the position from which the arc is observed, but the times at which the light maxima occur relatively to the times at which the current is a maximum will depend on this position.

Thus besides the rotation of the humming arc and the variation of the current observed by Mr. Trotter, I find that the light and P.D. vary with the same frequency, so that in the humming arc the frequencies of the rotation of the arc, and of the variations in the P.D. current, and light emitted in a given direction, are identical with the pitch of the note given out.

HISSING.

It has been shown by Messrs. Frith and Rodgers[1] and by Messrs. Duddell and Marchant,[2] that when a direct-current arc supplied from a constant source hisses, the current through it and the P.D. between its terminals vary rapidly; and M. Blondel[3] and Mr. Brown[4] have also found that the light emitted varies.

If the current through the humming arc be increased until the arc hisses, the variations in P.D., current, and light change, I find, in a most striking manner from the regular periodic variations of Fig. 5 to the very irregular variations shown in Figs. 6 and 7.

In spite of the very irregular nature of the variations, which irregularity is not surprising in view of Mrs. Ayrton's explanation of the cause of hissing given before this Institution last year, I think that they can be separated into two kinds, a large comparatively slow variation, and a rapid superimposed one. The light given out is alternately bright, with rapid variations in intensity, *a* to *b* Fig. 6, and dull with hardly any variations, *b* to *c*; the slow varia-

[1] *Proceedings of the Physical Society*, 1896, vol. xiv., p. 320.
[2] *Journal of the Institution of Electrical Engineers*, 1899, vol. xxviii., p. 86.
[3] *La Lumière Electrique*, 1892, vol. xliii., p. 54.
[4] *Physical Review* p. 210.

tion of the light corresponding with the larger variation of the current : the maximum light and current do not, however, occur simultaneously.

In view of the explanation given in the case of the humming arc that the large variations of the light is due to its rotation, and in view of the fact that the hissing arc is also probably rotating, as pointed out by Mrs. Ayrton, I think that the larger variations of P.D., current, and light in the hissing arc must also be due to the rotation of the arc. If this is the case, then the brighter parts of the curves are produced by light from the + crater and the rapid variations of density chiefly present in these parts of the lines are due to the rapid variation of this light from the + crater. [Experiment.]

Now these rapid variations in the light correspond with the small rapid changes in the current and the P.D., so that *the rapid variations of P.D. and current correspond with the variations of the light emitted by the + crater, and the large slow variations with the rotation of the arc as a whole.*

Considering one of the larger light maxima, say from *a* to *b*, or *c* to *d*, Fig. 6, during which the oscillograph mirrors receive the light from the + crater without being obstructed by the carbon, it will be seen that in many cases the maximum light and minimum P.D. occur practically at the same instant, whilst the *maximum current occurs later than the light maximum.* This is the opposite to what occurs when the current through the arc is varied by any *change in the circuit,* for in this latter case *the maximum current occurs before the light maximum.*

The periodically recurring sequence of events in the hissing arc is thus probably as follows, putting aside the rotation of the arc as a whole. Owing to the crater becoming too large for the end of the + carbon, the air obtains access to the crater surface as found by Mrs. Ayrton, the oxygen of the air there combines with the carbon, causing a rise of temperature, an increase of brilliancy, a drop in the P.D., followed very slightly later by a rise in the current.

I think that the above observations on hissing and humming are explained by, and confirm, the fundamental nature of Mrs. Ayrton's discovery of the cause of the hissing of the arc.

[illegible] of a transverse
[illegible] conditions, relight

itself; and if the blowing be continued, the arc will be extinguished and relight itself again and again with great rapidity, giving out a harsh sound. The rapidity of these intermittances may be very great; M. Blondel[1] has found them to be as high as 3,000 to 4,000 per second in the case of the alternate-current arc, and M. Abraham[2] has obtained 100,000 per second in the case of the flame discharge.

It was suggested by Professor Fitzgerald that this intermittance of the arc might be used to produce some high-frequency alternating current which I required. I therefore tried rendering a direct-current carbon arc in series, with a self-induction intermittent by means of a magnet. With this arrangement the rate of intermittance was irregular and not very high, probably owing to the E.M.F. of my source of supply being too low, although E.M.F.'s up to 300 volts were employed.

In order to try and overcome this irregularity, I connected a condenser (about 5 mf.) between the terminals of the arc, when to my surprise I found that the direct-current arc was intermittent even when *not* blown in any apparent way either by a stream of gas or by a magnetic field, and further that no self-induction in series with the arc was necessary.

Here then was a puzzle—a direct-current solid arc burning under ordinary conditions with resistance in series, and supplied with current from accumulators, became intermittent and gave out a musical note on simply shunting the arc with a condenser.

Leads were, of course, employed to connect the condenser as a shunt to the arc, and on twisting these leads together so as to destroy the small amount of self-induction which they possessed, I found that the musical note stopped, to be started again on separating the leads; and on interposing in the condenser circuit a loose coil of wire, the sound was greatly magnified. Hence the true statement of the facts is that given below.[3]

[1] *La Lumière Électrique*, 1893, vol. xliii., p. 54.

[2] *Société Française de Physique*, "Séances," 1899, ii. p. 70.

[3] NOTE (*added February 1st*, 1901).—Since writing the above Professor Elihu Thomson has written to me and to *The Electrician* pointing out that he carried out practically identical experiments as early as 1892, and patented this method for producing Alternating Currents. I regret that I was unaware of these experiments at the time of writing the paper, and so omitted to give Professor Thomson credit for them. Professor Thomson's letter, an editorial note on his patent specification, and a reply from me, appear in *The Electrician* for January 18th, 1901, vol. xlvi., p. 477.

MUSICAL ARC.

A direct-current arc of suitable length and current, between solid carbons, will give out a musical note if it be shunted with a condenser in series with a self-induction, as in Fig. 9, even though the source of supply of the current be perfectly constant and the arc be protected as far as possible from any outside cause of disturbance. [Experiment.]

I find that the musical note is produced by oscillatory currents flowing in the circuit composed of the condenser F, the self-induction L, and the arc, Fig. 9, and its pitch is determined by the periodic time of this circuit—that

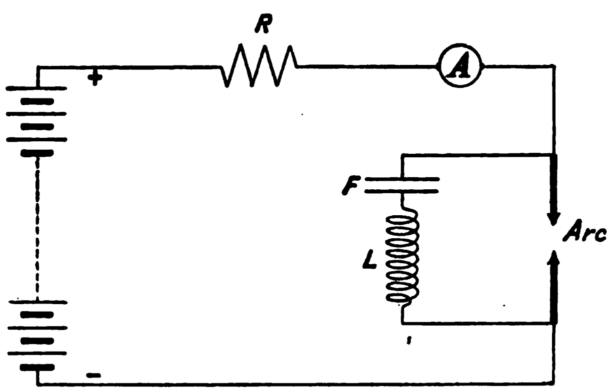


FIG. 9.

is, on the relation between the capacity, self-induction, and effective resistance of the circuit. Neglecting the resistance, which it will be shown later must be small, the periodic time of the circuit $\tau = 2\pi\sqrt{L.F.}$, and this has been found, by judging the pitch of the note by ear, to be approximately correct, so that for lecture purposes Kelvin's law can by this means be easily demonstrated. (See Appendix III.)

It must be remembered that although we have an alternate current through the condenser and self-induction, the source of supply is not an alternating one, and that *it is the arc itself which is acting as a converter and transforming a part of the direct current into alternating, the frequency of which can be varied between very wide limits by altering the self-induction and capacity*. The upper limit I find to be about

10,000 ∿ per second, and the lower limit, if such exists, is well below 500 ∿ per second.

It has long been known that a train of oscillations of almost any frequency can be obtained on discharging a condenser through a suitable inductive resistance, but of course these oscillations have a rapidly decreasing amplitude; and the means of supplying energy to such a circuit so as to maintain *the amplitude of the swings constant*, other than by means of a varying source of power having the same periodic time as the circuit, has been wanting. It is, therefore, necessary to inquire under what conditions it is possible for the arc to cause the source of direct current to supply the energy necessary to maintain the oscillations in the condenser circuit when once they have been started.

If the resistance in the main circuit in series with the arc is large, and if δV be a small instantaneous change in the P.D. between the terminals of the arc, δA the corresponding small change in the current through it, and r the resistance of the condenser circuit, not including the condenser; then, during the time this small change lasts, sufficient energy may be supplied to the condenser circuit to make up for the energy dissipated there, in ohmic losses, if the following conditions are fulfilled (see Appendix II.):—

1. $\frac{\delta V}{\delta A}$, negative.
2. $\frac{\delta V}{\delta A}$, numerically greater than r.

The question is, can the arc fulfil these two conditions? Messrs. Frith and Rodgers[1] have experimentally determined the value of $\frac{\delta V}{\delta A}$, which they call the resistance of the arc, for various arcs, and they found that while $\frac{\delta V}{\delta A}$ was always + when *both* carbons were cored, it was, on the contrary, always − when *both* carbons were solid; and that it was as small as −2 ohms for a 4-ampere solid arc. Now the resistance of the condenser circuit, r, external to the condenser, can easily be made less than 2 ohms, so that the arc can fulfil both the necessary conditions.

[1] *Proceedings of the Physical Society*, 1896, vol. xiv., p. 307.

I will now describe some observations on the musical arc which tend to confirm the above conclusions.

Arcs between solid carbons for which $\frac{\delta V}{\delta A}$ is always negative *work well*, while those between *cored* carbons for which $\frac{\delta V}{\delta A}$ is positive I find *will not work* under any conditions. [Experiment.]

The largest negative value of $\frac{\delta V}{\delta A}$ given by Messrs. Frith and Rodgers is 2 ohms for a 4-ampere solid arc, and it is probable that it did not exceed 2·5 ohms. for the smaller currents, viz., 3 to 3·5 amperes, which I used. According to the above conditions, 2·5 ohms should be the limiting resistance of the condenser circuit; by experiment it was found that when the resistance of this circuit was increased to 2·4 ohms the oscillations stopped and could not be restarted. [Experiment.]

It is evident that besides the resistance there are other causes, such as hysteresis, which tend to dissipate the energy in the condenser circuit and stop the arc giving its note. The hysteresis in an iron-wire core introduced into the self-induction will instantly stop the note. [Experiment.] Any complete circuit such as a ring of wire placed near the self-induction has the same effect. [Experiment.]

On several occasions before the importance of these causes of the dissipation of the energy were realised, considerable trouble was experienced in tracing the reason of the arc failing to give its note. As examples, in one case it was traced to an ammeter and in another to the tinfoil in the condenser which were acting as short-circuited secondaries to the self-induction coil, which had been placed too near them. [Experiment.]

The relation between the self-induction, capacity, and frequency can be very easily demonstrated by playing a tune on the arc by varying either the capacity or the self-induction by means of a key-board. [Experiment.] (See Appendix III.) Another method of varying the self-induction is by separating or bringing closer together the turns of the coil, as if playing on a concertina, the relative positions of the ··ns determining the self-induction and the pitch of the

The musical arc can be used as a means of com-

paring self-inductions or capacities by comparing the pitch of the notes produced.

The "enclosed arc" will work equally as well as the open arc, though the note given out is not so audible owing to the globe; but it can easily be made so by taking advantage of some of the telephoning effects mentioned in Part I.

The alternating current through the condenser circuit may be as large as from 3 to 5 amperes R.M.S. value, and the direct current in the main circuit also varies considerably depending on the amount of resistance in the circuit. This condenser current is sufficient to show experiments with alternating currents which do not require much power, and is very convenient in many cases for lecture purposes as the frequency, and any changes in it, are at once evident from the pitch of the note given out by the arc. Magnetic space telegraphy can easily be demonstrated on a small scale by using the self-induction coil as the transmitting circuit. [Experiment.] Several arcs can be used in series when more power is required in the condenser circuit than can be obtained from one arc alone.

TABLE OF DATA OF MUSICAL ARCS.

	Open Arc.	Enclosed Arc.
Carbons both solid.	*Conradty.*	*Electra.*
Diameter	9 mm.	13 mm.
Arc Length	1·5 mm.	1·0 mm.
„ Current	3·5 amps.	5 amps.
Resistance in Series R. ...	42 ohms.	about 28 ohms.
Self Induction of L.	$5·3 \times 10^{-3}$ h.	$5·3 \times 10^{-3}$ h.
Resistance of L. and Leads ...	0·41 ohms.	0·41 ohms.
Capacity of Condenser F. ...	1·1 to 5·4 mf.	1·1 to 5·4 mf.
R.M.S. Current through Condenser when Capacity = 5·4 mf.	3 amps.	4·5 amps.

For the convenience of those who may wish to repeat these experiments, I have inserted a table of good working

conditions for open and enclosed arcs. The exact figures need not be strictly adhered to, as the musical arc will work over a wide range of conditions. It may perhaps be well to mention that only condensers suitable for high voltages should be used, as although the P.D. arc is only 50 volts, the P.D. condenser rises to several hundred volts.

METAL ELECTRODES SWITCH CONTACTS.

In connection with the above experiments the attempt was made to replace the carbons by metal electrodes, when I found that on trying to shunt the metal arc with a condenser it went out, no self-induction except that of the leads being used. [Experiment.] Of course, whether the arc is extinguished or not depends on the capacity used to shunt it and on the other conditions of the circuit; thus in the present case, with a 3-ampere arc between 6 mm. diameter copper electrodes and a resistance in series of from 50 to 60 ohms, supply voltage 200, it was found that the arc was always extinguished when shunted with a condenser having a capacity from 0·6 to 5·4 mf., though with the smaller condenser, 0·6 mf., and longer arc lengths the extinguishing was not quite so certain. Condensers larger than 5·4 mf. were not tried, though I have no doubt that they would prove even more effective.

This experiment is very instructive as showing how very soon the metal arc becomes practically non-conducting after the current through it is interrupted, for if we consider that the current through the arc is reduced to zero at the instant of first connecting the condenser, and remains zero unless the arc relights, then the time required for the 0·6 mf. condenser to charge up to $(1 - \frac{1}{\varepsilon})$, or 63 per cent. of the supply voltage, *i.e.*, 126 volts, is about $\frac{1}{27000}$th of a second. So that we may consider that if the current through the metal arc is interrupted for about one twenty-seven thousandth of a second, even applying about three to four times the normal voltage,[1] will not cause it to relight. This is very different from the case of the arc between cored carbons, for it is well known that the current through a 10-ampere cored arc may

[1] Direct-current metal arcs as above usually require a P.D. roughly about 30 volts.

be interrupted by opening a switch in series with it for, say, a quarter-second, and yet the arc will relight on closing the switch again, owing to the high conductivity of the vapour left, when the arc is extinguished. The comparison is, however, not quite a fair one, as it might be expected that with the larger current, viz., 10 amperes used with the cored arc, more conducting vapour would exist than with the 3 amperes used for the metal arc, and that it would therefore take longer for the vapour column of a 10-ampere arc to cool down, and attain a high resistance than that of a 3-ampere arc.

In order to make a fair comparison, the metal electrodes were replaced by cored carbons and a 3-ampere arc obtained under as nearly as possible the same conditions as the copper arc. This cored carbon arc could not be extinguished even on shunting it with the largest condenser, viz., 5·4 mf. [Experiment], and it was found necessary, in order to make the cored arc go out on shunting, to reduce the current through it to below 1 ampere; but with such a small current the arc is rather unstable and liable to go out even when not disturbed in any way. Two solid carbons were also tried, and the effects were found to be intermediate between the cored arc and the metal arc, as a 2-ampere solid arc could just be put out by shunting with the 5·4 mf. condenser, whereas the 3-ampere metal arc always went out on being shunted with a condenser of as small a capacity as 0·6 mf., as already stated.

The correct method of finding out whether the arc will relight in any given case after it has been extinguished on suddenly reducing the current through it, is the following:—Let A, Fig. 10, be a curve which might be drawn between the P.D. which will have to be set up between the electrodes to relight the arc, and the time that has elapsed since the arc was extinguished; and B the curve that connects the actual rise in P.D. between the electrodes (*i.e.*, between the condenser terminals) and the same time. Then the condition for the arc to relight is that the curve B touches or cuts the curve A.

Unfortunately we do not know much about the curve A between P.D. required to relight the arc and time except that it starts from the P.D. at which the arc was burning at the instant it was extinguished, and attains a final constant

value equal to the P.D. required to spark across between the electrodes. We can, however, form some idea of the steepness of the curve A at the commencement, for we know that, if the arc fails to relight, the curve A lies between the ordinate at the time of connecting the condenser and the curve B, that is the ordinate at time nought. The shape of this latter curve, which represents the P.D. between the terminals of the condenser during charge, can be calculated from the known data of the circuit; thus with the copper arc mentioned above, which is just extinguished by shunting

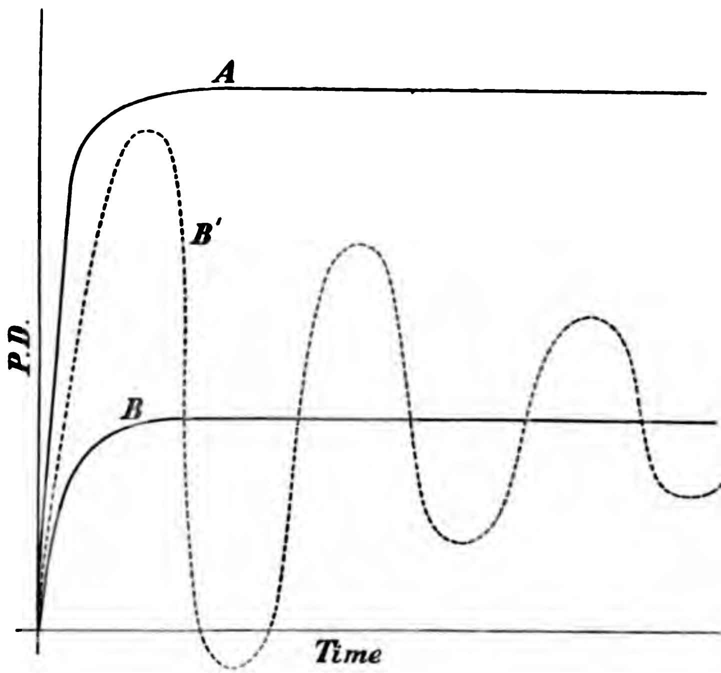
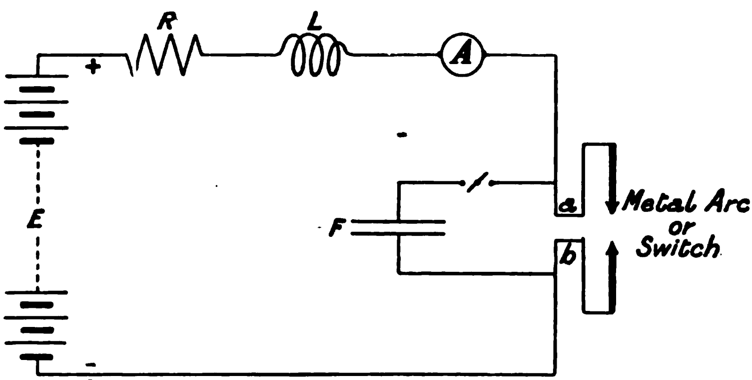


FIG. 10.

with a condenser of 0·6 mf. capacity, E.M.F. in circuit being 200 volts, resistance 56 ohms, and self-induction of leads neglected, the curve B will start with an initial steepness of about 6 × 10^6 volts per second. In spite of this very rapid rise of curve B, it will generally fail to intersect the curve A for the 3-ampere copper arc, so that the apparent resistance of the copper arc seems to increase at a very high rate after the current through it is stopped.

With cored carbon electrodes the arc under similar conditions could not be extinguished by shunting with 5·4 mf., so that since the initial steepness of the curve B was $\frac{1}{9}$th, or about 7×10^5 volts per second, this curve always intersected the curve A for cored carbons. Further, I think that the curves would still intersect, that is the cored arc would relight, if the initial steepness of B had been even many times smaller, so that the rate of increase of apparent resistance of the cored arc after interruption of the current is many times smaller than with the copper arc. In what has been said above, I have neglected the unknown self-



E = 200 volts. *L* = 3 henrys (about)
R = 60 ohms (about). *F* = 5·4 microfarad.
Current = 3 amps.

FIG. 11.

induction of the leads, so that the figures given must be considered as only rough approximations.

The extreme rapidity with which it is necessary to increase the P.D. between the terminals of the metal arc in order that it may relight again after the current through it has been stopped, explains the fact that it seems impossible to maintain an *alternate* current arc between *metal* electrodes at ordinary frequencies and P.D.s of even several hundred volts; and that it requires a P.D as high as 2,000 volts to maintain a metal arc as found by Herr Arons.[1]

If the non-inductive resistance in series with the arc be replaced by a highly inductive one as shown in Fig. 11, the

[1] *Wiedemann's Annalen*, vol. lvii., p. 185.

curve B will be altered in shape, and with the conditions inserted under Fig. 11 the charge of the condenser will be oscillatory as shown by B′ Fig. 10, the maximum P.D. attained if the arc fails to relight at all being many times as high as the E.M.F. of the source of supply. Although the arc is put out on shunting with a condenser, it does not follow that it may not have really relit and gone out again several times corresponding with each swing of the condenser, before it is finally left extinguished owing to the dying away of the oscillations; and this is probably what occurs when the curve A is not very steep, as in the case of cored carbons. In this case the maximum rise in P.D. will be limited by the shape of the curve A and by the amplitude of the oscillations being rapidly damped, due to conduction through the arc.

This high rise in P.D.—caused by the sudden annulling of the current through the self-induction when the metal arc is extinguished on shunting it with a condenser—is very serious, as the following experiment shows. A 3-ampere arc between two copper electrodes 6 mm. diameter, the conditions of the circuit being those given under Fig. 11, was shunted with a condenser 5·4 mf. capacity. This caused the arc to go out and so high a rise in P.D. to be produced that the insulation of the leads broke down, a spark passing from *a* to *b*, accompanied by a report. [Experiment.] When, however, I substituted carbons for the copper electrodes, no report was heard, nor was any serious rise in P.D. noticed. [Experiment.]

The next experiment tried was to connect the condenser permanently as a shunt to the metal electrodes, and then to attempt to strike the arc, the circuit being arranged as in Fig. 11. I found that it was impossible to strike an arc between metal (Cu, Fe, Al, and Brass) electrodes if the capacity of the condenser F, Fig. 11, exceeded 0·1 mf.—even although an E.M.F. of 200 volts was used—and that on separating the electrodes the sudden interruption of the current through the self-induction set up oscillations in the circuit and a high rise in P.D. between the terminals of the condenser similar to that produced when the metal arc was extinguished by shunting with a condenser as explained above. The condition that determines the possibility of striking the arc is similar to the condition that governs the

relighting of the arc after the current through it has been reduced to zero as explained above. For corresponding with each position of the electrodes as they separate there is a certain P.D. required to start the arc, and if the relation between the position of the electrodes and time be known, then a curve between P.D. required to start the arc and time can be plotted similar to curve A, Fig. 10 above, and the intersection or otherwise of this curve with the curve B′ determines whether the arc will strike or not.

The practical interest in this subject of the striking of the arc lies in the fact that when the attempt is made to interrupt a steady direct current flowing through an inductive circuit by means of a switch with metal contacts, an attempt is really made, at the first instant, to strike a metal arc between the contacts, and if these contacts be shunted by a condenser which prevents the arc from forming, a high rise in P.D. will occur. If, however, the arc was allowed to form, the time during which the break takes place would be lengthened, and no such great rise of P.D. would be produced. This rise in P.D. has been mathematically investigated by Mr. Johnson[1] on the assumption that the arc does not form, and putting the data given under Fig. 11 into his equation, I find that the rise in P.D. is just over 2,000 volts as compared with 200 volts the E.M.F. in the circuit. It is, therefore, of importance when it is required to prevent these rises in P.D., on breaking the circuit to so choose the substance of the switch contacts that the arc shall not be prevented from forming or be suddenly extinguished by the action of the condenser, that is to say *that arcing at the switch contacts should rather be encouraged than otherwise*, of course always supposing that no special method such as a non-inductive resistance shunting the switch be provided to dissipate the energy stored in the self-induction.

The following experiments illustrate the importance of the nature of the switch contacts and of the condenser which shunts them. The circuit used was that shown in Fig. 11, the arc being replaced by a switch with brass contacts, and the data of the circuit being those given below the figure. When the condenser F was disconnected, and the insulation between *a* and *b* was made to consist of a single thickness of paper, I found that the circuit might be

[1] *The Electrician*, 1900, vol. xlv., p. 281.

made and broken by means of the switch, either quickly or slowly, without the paper between a and b being pierced. [Experiment.] After reconnecting the condenser F, however, every time the switch was opened the paper was pierced, and even three thicknesses of the paper could not withstand the rise in P.D. that occurred. [Experiment.] The steady P.D. required to pierce one thickness of the paper was found by a separate experiment to be about 550 volts, and that required to pierce three thicknesses about 1,500 volts, so that without the condenser shunting the switch the rise in P.D. on breaking the inductive circuit was under 500 volts, but with the condenser as a shunt to the switch was over 1,500 volts, showing that the metal arc must have been almost completely suppressed, as the maximum value of the P.D. calculated above on the assumption of no arc forming at all was only just over 2,000 volts.

The influence of the nature of the contacts of the switch on the rise in P.D. which occurs when the switch is shunted by a condenser is very marked; thus with the metals, copper and brass, serious rises in P.D. were always found to occur, with solid carbons as contacts the rise was much less, and with cored carbon contacts was inappreciable. [Experiment.]

Breaking the circuit between metal contacts under tap-water, or shunting the metal contacts while in air by wires dipping into water, also prevented any serious rise in P.D.

It was also found that resistance or self-induction introduced into the connections between the condenser and the contacts greatly reduced the rise in P.D. on opening the switch.

I attempted to use an electrostatic voltmeter to measure the rise in P.D. instead of the rough method of the piercing of paper, but although the voltmeter was sufficiently sensitive to read steady P.D.s much below that required to pierce the paper, it failed to indicate the rises in P.D. This is probably due to the short time the rise in P.D. lasts.

There are two practical cases in which capacity shunts the switch contacts to which I will refer.

The first is the ordinary induction coil in which the circuit is the same as Fig. 11, the switch being replaced by the contact m[illegible] arc. In this case a high rise in P.D. is

required so that the nature of the contact points should be such that the arc can be completely extinguished by as small a condenser as possible; for the rise in P.D., if the arc is completely extinguished, will be the higher the smaller the capacity of the condenser. (See also Appendix IV.) It is evident, therefore, that carbon would be very unsuitable for the contacts of an induction coil. This has lately been shown to be the case by the experiments of Mr. Beattie,[1] who finds that with a slow break the maximum length of spark obtainable between the terminals of the secondary, using *platinum contacts*, is nearly $2\frac{1}{2}$ times that obtainable when *carbon contacts* are used, the current interrupted at the break being the same in both cases. I think that if *cored carbons* had been used, a much greater disparity in the spark length would have been found. [Experiment.]

The second case is that of a switch or circuit breaker connected with a concentric cable so that the capacity shunting the contacts is supplied by the distributed capacity of the cable. Whether this distributed capacity in practical cases will have the same effect as a condenser shunting the contacts, as suggested by Mr. Johnson, is, I think, a matter for further experiment. If it has, then serious rises in P.D. are to be apprehended on interrupting a *direct* current, though an inductive circuit, by means of metal contacts, the capacity of the cable forming a shunt to the contacts.

Assuming this to be true for *direct* currents, may not some of the breakdowns of concentric cables supplying power by means of *alternating* current be also due to the sudden quenching of the arc at metal contacts, and not to the fact that the current is an alternating one? I suppose, of course, that the attempt to interrupt the current is made at some point in the period when the current is large.

Before concluding this paper, I wish to express my indebtedness to Professor Ayrton and Mr. Mather, of the Central Technical College, not only for allowing me to carry out the experiments in the laboratories of the College, but also for the valuable assistance and advice they have given me during the course of the experiments. I also wish to express my thanks to the many students who have helped me from time to time, and especially to Messrs. Brown, Watson, and Fithian.

[1] *Phil. Mag.*, 1900, vol. l., p. 146.

Conclusions.

If the current be suddenly increased through a direct-current arc between two solid carbons, the P.D. and current increase together for less than about $\frac{1}{5000}$ second, and at the end of this very short time the P.D. decreases with an increase of current in the ordinary way.

If the current through a direct-current arc varies by as little as 3 per cent. from the mean, and if the frequency of these superimposed variations is even as high as 4,300 $\sim$ per second, a variation in the light emitted by both the + crater and the vapour column can be detected.

A rapid periodic variation of the order of one part in 10,000 from the mean current will alter the vapour column of the arc sufficiently to produce sound-waves; and a variation of one part in 100 will produce sound-waves even at frequencies as high as 30,000 $\sim$ per second.

The arc is affected by such small changes of outside conditions as sound-waves produce.

The direct-current arc can be used both as a telephone receiver and transmitter.

In the direct-current humming arc the P.D. current and light emitted vary periodically, the frequency of these variations being the same as that of the rotation of the arc as a whole, and of the pitch of the sound emitted.

In the direct-current hissing arc the P.D. current and light emitted vary very irregularly, the larger and slower variations corresponding with a rotation of the arc as a whole and the smaller and more rapid to the hissing proper, *i.e.*, the oxygen of the air obtaining access to the crater surface as demonstrated by Mrs. Ayrton.

Under certain conditions the direct-current solid arc will emit a musical note when shunted by a self-induction in series with a condenser.

When emitting the musical note, the direct-current arc transforms direct-current energy into alternate-current energy, the frequency of the latter being determined by the self-induction, capacity, and effective resistance of the oscillating circuit. The pitch of the note emitted may be used as a means of comparing self-inductions and capacities.

If a direct-current arc be shunted with a condenser of ·al microfarads capacity, the arc will generally be ex-

tinguished if the electrodes are of metal, and not if they are of cored carbon, the resistance in series with the arc being non-inductive.

If the resistance in series with the arc be highly inductive, then, when the metal arc is extinguished by shunting it with a condenser, a violent rise in P.D. occurs between the terminals of the arc.

The rise in P.D. that occurs when an inductive circuit is broken by means of a switch, the contacts of which are shunted by a condenser, is much higher if their contacts are of metal than if they are of cored carbons, owing to the condenser extinguishing the metal arc formed at the contacts more suddenly than the arc formed when carbon contacts are separated.

APPENDIX I.

ON THE RESISTANCE OF THE CORES OF CORED CARBONS.

I do not remember having seen it pointed out that the much greater stability of arcs between cored carbons than of those between solid carbons can not be very well due to the high conductivity of the material of the core, while in place in the carbon, for the cores have generally a higher specific resistance than the solid carbon which surrounds them, as the following experiment shows :—

Three carbons were taken—two cored and one solid—of the same nominal diameter (11 mm.), and a current of 9·9 amperes was passed through them. The drop of volts was measured along a length of 20 cms. of each after they had attained a steady temperature.

Each of the three carbons then had a hole 3·16 mm. diameter drilled through it so as to completely remove the cores of the cored carbons and the centre of the solid carbon, and the drop of volts was remeasured as before. The results are given in the table below, from which it appears that drilling a hole in the solid carbon increased its resistance 7·8 per cent., whereas drilling the same sized hole (which removed the core and a small amount of the solid carbon) in a cored carbon of the same make only increased its resistance by 2·1 per cent.

Allowing for the fact that a small quantity of solid carbon was removed along with the core in drilling, *the specific resistance of the core, of one make of cored carbon, was about sixteen times that of the surrounding solid carbon, and in the other the specific resistance of the core wa practically infinite.*

Make of Carbons ...	"Apostle" Solid.	"Apostle" Cored.	"Brush" Cored.
	mm.	mm.	mm.
Mean diameter	10·07	10·05	10·70
Mean diameter of core	—	2·84	2·82
Drop of volts along 20 cms. *before* drilling ...	1·71	1·74	1·52
Drop of volts along 20 cms. *after* drilling ...	1·84	1·77	1·56
Per cent. increase of resistance due to drilling..	7·8	2·1	2·4
Ratio $\dfrac{\text{Specific resistance of core}}{\text{Specific resistance of surrounding solid carbon}}$	about	∞	16

APPENDIX II.

On the Conditions which Govern the Conversion of Direct Current into Alternating Current in the Musical Arc.

(See Fig. 9.)

Let E and C be the E.M.F. and current through the cells, when there is no oscillatory current through the condenser circuit.

Let V and A be the P.D. and current through the arc under the same conditions.

Let R be the resistance in series with the arc, including that of the cells.

Let r be the resistance of the condenser circuit.

Let δV be a small change in the P.D. arc which produces a current δi through the condenser circuit for a time δt, and let δV and consequently δi be assumed to change sign at the end of each interval of time δt.

Let δA and δC be the corresponding changes in A and C; E being assumed constant.

The energy supplied to the condenser circuit—

during one interval $\delta t = (V + \delta V)(+\delta i)\,\delta t$

" next " $\delta t = (V - \delta V)(-\delta i)\,\delta t$

Total during one complete period $2\,\delta t = 2\,\delta i\,\delta V\,\delta t$

Energy dissipated in ohmic losses during $2\,\delta t = r(\delta i)^2\,2\,\delta t$

In order that, during each complete period $2\,\delta t$, energy may be supplied to the condenser circuit, we must have

$$\delta i\,\delta V \text{ positive.}$$

And in order that this supply shall make up for the ohmic losses we must have

$$\delta i\,\delta V \geq r(\delta i)^2$$

Now

$$\delta C = \delta A + \delta i$$

$$\text{and } C = \frac{E - V}{R}$$

$$\therefore \delta C = \frac{-\delta V}{R}$$

$$\text{and } \delta i = -\frac{\delta V}{R} - \delta A = -\left(\frac{1}{R} + \frac{\delta A}{\delta V}\right)\delta V$$

$$\delta i\ \delta V = -\left(\frac{1}{R} + \frac{\delta A}{\delta V}\right)\left(\delta V\right)^2$$

$\therefore$ for a supply of energy to condenser circuit $\frac{\delta V}{\delta A}$ must be negative and numerically less than R.

Supposing $\frac{\delta V}{\delta A}$ negative, then in practice the second condition is always fulfilled, or $\frac{\delta V}{\delta A} + R$ would be negative and the whole circuit unstable.

Next the condition that sufficient energy be supplied to make up for the ohmic losses gives

$$\delta i\ \delta V \geq r(\delta i)^2$$

and as $\delta i\ \delta V$ is positive,

$$r\frac{\delta i}{\delta V} \leqslant 1$$

$$-r\left(\frac{1}{R} + \frac{\delta A}{\delta V}\right) \leqslant 1$$

$\therefore$ to obtain best supply of energy to condenser circuit we require R very large and r very small.

Suppose $\frac{1}{R}$ may be neglected, compared with $\frac{\delta A}{\delta V}$ then condition becomes

$$-\frac{\delta V}{\delta A} \geq r$$

Thus it is possible if $\frac{\delta V}{\delta A}$ is negative and numerically greater than r, for the condenser circuit to receive sufficient energy during each very small complete oscillation to compensate for the energy dissipated in ohmic losses during the oscillation. For larger oscillations, similar but more complicated expressions will probably be required.

APPENDIX III

On the Relation between the Pitch of the Note and the Capacity and Self-Induction Shunting the Musical Arc

In order to demonstrate that the pitch of the note emitted by the musical arc is determined by the capacity and self-induction of th

circuit shunting it, and is given by the formula frequency of note emitted $= \frac{1}{2\pi\sqrt{LF}}$, when the resistance of the circuit is negligible, a series of capacities was calculated by means of this formula, to give one octave of the diatonic scale, on the assumption of a constant self-induction L. A fairly close approximation to this series of capacities was obtained by combining in parallel condensers chosen from a set of eight Swinburne condensers, the capacities of which are given below. A keyboard was constructed which made the necessary connections. The condensers used in parallel and their actual capacities are tabulated against the notes on the keyboard they were respectively intended to produce. This arrangement of condensers and keyboard, arrived at entirely by calculation, was that used to play tunes on at the meeting.

The self-induction L consisted of a coil of about 40 lbs. of No. 10 D.C.C. copper wire coiled into a coil of about 18 inches diameter, having a resistance of about 0·44 ohm and a self-induction of 14·75 henrys.

The frequency of each of the notes emitted by the arc was determined for me by Mr. G. Wall and Mr. L. Murphy by comparing, by means of a monochord, each note with a standard tuning-fork giving 512 complete vibrations per second. The frequencies so determined are tabulated below along with the frequencies calculated by means of the formula from the known self-induction and the capacity in each case. The agreement between the two demonstrates, I think, fairly conclusively that the pitch of the note is determined by the periodic time of the circuit shunting the arc. It will be noticed that the calculated frequency is in most cases about 1 per cent. higher than the observed; this is probably due to the fact that in calculating the frequencies no account has been taken of the resistance of the circuit in which the oscillating currents are flowing, as this resistance should include that of the arc. This is borne out by the fact that the note depends to a slight extent on the length of the arc and on the current through it. Another possible cause of the difference may be that the capacities of the condensers used may not be quite the same at these high frequencies, 550 to 1,100 ∿ per second, as at 100 ∿ per second, the frequency at which they were determined.

LIST OF SWINBURNE CONDENSERS.

Called.	Capacity in Mfs.	Called.	Capacity in Mfs.
a	2·515	*e*	0·307
b	1·142	*f*	0·119
c	1·146	*g*	0·130
d	0·612	*h*	0·057

Data of Condensers for Musical Arc.

Note on Keyboard.	Condensers Used in Parallel.	Capacity in Mfs.	Frequency ∿ per Second. Calculated.	Frequency ∿ per Second. Observed.
C	*a, b, c, d, g*	$5{\cdot}54_5$	558	545
D	*a, b, d, g*	$4{\cdot}39_9$	624	618
E	*a, d, c, f*	$3{\cdot}55_3$	695	688
F	*a, d*	$3{\cdot}12_7$	740	735
G	*b, c, g, h*	$2{\cdot}47_5$	832	822
A	*b, d, f, g*	$2{\cdot}00_3$	926	915
B	*b, e, g*	$1{\cdot}57_9$	1042	1045
C	*b, f, g*	$1{\cdot}39_1$	1110	1101

To increase the alternating current in the condenser circuit and the loudness of the notes emitted, it is necessary to increase the number of

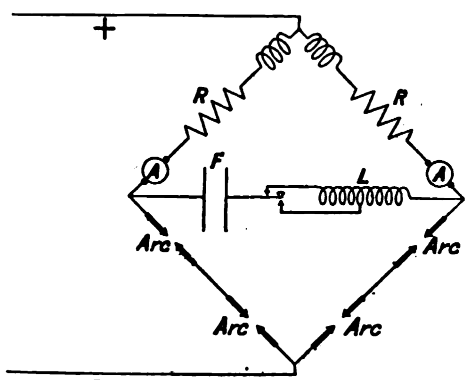


Fig. 12.

solid arcs used in series. With the ordinary 200-volt supply only three solid arcs can be run in series, and even this number gives trouble owing to want of sufficient stability. In order to overcome this difficulty I employed the arrangement shown in Fig. 12, by means of which all the advantages in stability of having only two arcs in series are secured at the same time that, as far as the condenser circuit is concerned, we have four arcs in series and the consequent increase in the alternating current and the loudness of the notes emitted. As used at the meeting the data were as follows: Pressure of supply 200-

volts current through each arc 5 amperes. Carbons + 11 and − 9 mm. solid "Apostle." Arc length about 1 mm. F condenser and keyboard described above. L self-induction described above, with an extra point brought out so as to reduce its value to ¼ and give a second octave.

APPENDIX IV.

(*Communicated February* 22, 1901.)

During the experiments on the shunting of a metal arc with a condenser, I noticed that the rise in P.D. which occurs between the terminals of the arc when the circuit is inductive always seemed to be greater when the condenser was applied as a shunt to an arc after striking the same than when the electrodes were first shunted with a condenser and afterwards the attempt was made to strike the arc. Prof. Fitzgerald suggested to me, quite independent of the above observation, that in the case of an ordinary induction coil an increase of spark length might be obtained if instead of connecting the condenser, as is usual, permanently as a shunt to the contact points the condenser was disconnected during the time the contact points separate to a small distance, and was then connected as a shunt to them so as to suddenly extinguish the small arc or spark that had formed between them.

In order to carry out this suggestion a contact maker driven by a motor was constructed by means of which the condenser could be connected as a shunt to the contacts, which acted as the interrupter for the primary current of the induction coil either before their separation (ordinary method) or at any desired time interval after the separation of the contacts and the formation of the arc between them (new method).

With this apparatus, using a six-inch spark "Apps" induction coil the primary current being supplied by three accumulators and regulated by means of a carbon resistance, and using the condenser belonging to the coil, I found that if the primary current was adjusted to give the maximum length of secondary spark, first with the condenser applied before the separation of the contacts, second with the condenser applied a certain time after their separation, then the spark length in the latter case (new method) was from two-and-a-half to three times as great as in the former or ordinary way. Again, if, when the spark length was adjusted to its maximum value with the new method, the condenser was applied in the ordinary way without changing the current or the speed of the contact maker, then the secondary spark was reduced to about one-sixth or one-seventh the length, so that the new method of applying the condenser to the induction coil gives in both cases a considerable increase of spark length over the old.

An observation was made whilst working with this contact maker on the curious way in which a trace of oil on the contacts affects the secondary spark length. If, with the condenser connected in the ordinary way, the current through the contacts was so small that

practically no arc formed between them, then a trace of paraffin oil on the contacts *increased* the secondary spark length. If, on the other hand, the current was so large that appreciable arcing was taking place, then the oil on the contacts *reduced* the spark length, apparently due to the oil being decomposed, and introducing carbon vapour into the arc between the contact points, thus reducing the suddenness of the interruption of the primary current.

The President.

The PRESIDENT: It is quite evident we have not time for much discussion, and I am sorry to say the discussion on this paper cannot be adjourned. I think, however, we ought to call upon Professor Ayrton to say how it bears upon that negative resistance of his which was so much maligned some time ago.

Professor Ayrton.

Professor W. E. AYRTON: The paper which we have just heard read has given me exquisite pleasure: not because I have any claim to be its author, although I felt as pleased while I heard it read as if I had been the writer; nor is it merely because I feel convinced that these experiments of to-night will assist in the development of the electrical industry of to-morrow: it is rather because it so rejoices the hearts alike of professional men—yea, and of professors—to find a student who so resembles a solid carbon arc that he is ever on the alert to catch at and magnify any hint which may come from Nature or man. From Mr. Duddell's papers of two years ago, and of to-night, we learn much; among other things this second one has taught us how valuable was that research made some five years ago by Messrs. Frith and Rodgers. For what did that investigation really show us? It brought out an absolutely new fact. Supposing this is an alternating-current circuit (Fig. A), the alternator running at a given frequency and supplied with a given exciting current, the alternating current in the circuit being measured by an accurately graduated alternate-current ammeter, and that this is a wholly separate circuit—a direct-current circuit supplied by accumulators—and sending a direct current through a solid carbon arc. Then what they showed was this, that if you make a break in this alternate circuit and insert the solid carbon arc (Fig. B) without making any change in the resistance, speed, or excitation of the alternator, etc., you increase the alternate current, not merely the current when flowing in one direction—for the condenser stops any direct current—but you increase the current in *both* directions; that is, the alternate-current ammeter reads higher after the arc has been inserted than it did before, and higher than it will if the arc be short-circuited. Now that investigation was undertaken by these gentlemen because certain theoretical considerations led me to suggest that if the method that had been employed by various experimenters to measure the resistance of the arc—a method employed without comment or adverse criticism as long as a positive answer was obtained—was applied in a certain case to an arc a negative answer for the resistance of the arc would be found: that the method, in fact, which up to that period had been used successfully to give the resistance of the arc, and which had always given a positive value, I pointed out would give under certain conditions, a negative answer. Certain preliminary experiments

Professor Ayrton.

having been made by Mr. Mather which confirmed my idea, a long investigation was carried out by Messrs. Frith and Rodgers. And they found that whenever the arc was formed between *solid* carbons, the ratio of an instantaneous change of P.D. to the corresponding instantaneous change of current was *negative*, whereas if the carbons were *both cored* it was always *positive*.

A howl of indignant criticism followed. Had Messrs. Frith and Rodgers and I lived in the Middle Ages we should undoubtedly have been burned in the solid carbon arc. But there were three distinguished investigators who had the insight, who had the courage, not to be drawn into this net of conventional antagonism, and these were Professor

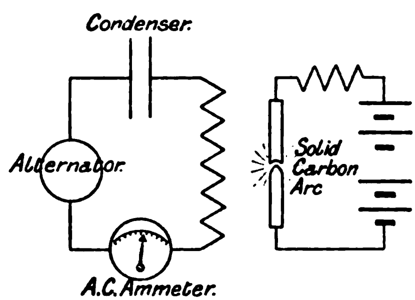


FIG. A.

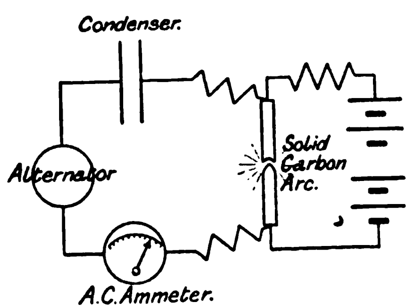


FIG. B.

Gray, then of Bangor, now Lord Kelvin's successor at Glasgow, Mr. Oliver Heaviside, and last but by no means least, the Chairman of your Dublin local section, Professor Fitzgerald, to whom we, like so many experimenters, are indebted for the many suggestions which he has made. And I, even I, ventured to suggest that progress would be more expedited, I thought, if the critics, instead of merely cavilling at both cases my of the sugar figure on the cake, would cut it open, and see An observation ere really any plums inside. It has remained, however, the curious way in we the real Jack Horner to put in his thumb and secondary spark lengt modestly leave you and me to finish the rhyme ordinary way, the curnboy he is."

Professor Ayrton.

I do not propose this evening to consider whether or not what Messrs. Frith and Rodgers measured was the true resistance of the arc, because opportunity, I hope, we shall soon have of going fully into the subject; but in justice to Messrs. Frith and Rodgers, and in virtue of the far-reaching principle that they really brought to light, I desire to emphasise what they really obtained. Mr. Duddell has taught us what was the real significance of their work. Our lost friend Professor Hopkinson, following Mr. Wilde, proved several years ago that two alternators could not be run in series. Thus, for example, if an alternator with a certain exciting current supplied to it, and driven with a steam engine, or an electric motor, or whatever it might be, were coupled mechanically to another similar alternator, driven by an independent steam engine, and speed were got up so that the two machines were going exactly in step and then were joined in series with some outside circuit, he proved to us that the moment this mechanical coupling was severed the two alternators would get out of step. So, as is well known, two alternators cannot run in series. And he might have added—if there had been the slightest necessity for him to do so—that if two alternators cannot continue running in series even if they have been started with the same frequency and exactly in step, still more impossible must it be for one alternator, driven by its own steam engine, to run in series with another independently driven when the frequency of the second is being altered within wide limits. Nevertheless, gentlemen, that is exactly what the arc does. For in Fig. B we have one alternator running in series, with a second alternator, viz., the solid carbon arc, supplying alternating current to the circuit—because the arc transforms direct-current energy into alternate-current energy—and does it in such a way that whether the frequency of this alternator be as Messrs. Frith and Rodgers found, seven periods per second, or 70, or 170, or 250, which was the highest limit which could be obtained in my laboratory at that time, and whatever the current might be, within the limits they tried, this gallant little alternator—the solid carbon arc—helps the other, and supplies current sufficiently in phase as to make the alternate current greater when it, the arc, is inserted than when it is taken away or short-circuited.

Mr. Duddell has not only pointed out the importance of that result, the novelty of that result, but he has pointed out something even further. He has shown us that an ordinary so-called perfectly silent arc supplied with current from accumulators is, if the carbons be solid, like the mouthpiece of a flageolet or flute, not blown. The application of a shunt to that arc, consisting of a capacity in series with a self-induction, performs two operations. It starts vibrations in the arc, just as blowing a flute gives rise to vibrations of many different rates. Just as one of those rates of vibration is picked out and reinforced in the case of a flute or flageolet by the form of the resonance chamber dependent on the position of your fingers or keys, so in this musical arc the particular one of the many vibrations that is probably started which is picked out and reinforced depends on the capacity of the condenser and the value of the self-induction which is in series with it.

Professor Ayrton.

Already he has shown you a practical result that has followed from this. I do not mean merely those illustrations which he has given of magnetic space telegraphy, and by means of which he has shown how it has become possible to easily and experimentally demonstrate that the E.M.F. of the current induced in the distant secondary coil is proportional to the frequency of the alternating current in the primary, which will be of marked value to teachers; but beyond that he has shown us how to much improve a well-known instrument. The other day he demonstrated in my laboratory that, adding to the ordinary circuit of that very induction-coil, joined up as usual, his little motor so as to put the condenser across the break *just after* the break was made, a spark was obtained from five to seven times as long as was obtained with the coil in the ordinary way. Even if you try to make the comparison absolutely fair, and try to arrange the very best conditions in each case for the old method and for the new method, you still find a great advantage. Let somebody take, for example, an induction coil and arrange the battery-circuit, contact-breaker, etc., in the old way as well as he can, and let Mr. Duddell use the same induction-coil, battery, condenser, etc., and deal with it as well as he can, then the spark produced in the latter case will be from 2¼ to 3 times as long as in the former.

There is one other point which has come out in connection with these experiments of a rather different kind. Some ten or more years ago a paper was read by Dr. Sumpner and myself before the Royal Society, pointing out what was then new, that in the case of an alternating-current arc, if we used solid carbons, the true power given to the arc, that is the power measured by some accurate method, was considerably less in some cases than the current measured by a good current ammeter multiplied by the pressure measured by an alternate-current voltmeter. Subsequently to that, in making experiments I was pretty convinced that under certain circumstances a direct-current arc behaved in the same way. I was pretty certain, in the case of a direct-current arc, especially when solid carbons were used and the arc was hissing, that the power-factor was no longer unity, but not sufficiently sure of the fact to publish it, especially in view of the way in which that other paper which I have just referred to was received at the Royal Society, and the scepticism which was raised when Dr. Sumpner and I pointed out how far from unity we found the power-factor was with certain alternate-current arcs. But now I am sure, from results of experiments which have been recently made in my laboratory, that with an arc supplied with accumulators, a so-called steady direct-current arc, if the carbons be solid and there is hissing, the power-factor may be several per cent. different from unity—that is, the true power as measured by a true instrument, for example a good wattmeter, may be several per cent. less than the product of the voltmeter reading into the ammeter reading.

When I read this paper of Mr. Duddell's, I thought he had proved that a solid carbon direct-current arc was the most sympathetic soul I had ever met, but you have convinced us that that is not quite the case, for you have shown Mr. Duddell this evening that the sympathy of his arc is ever [illegible] the sympathy of his audience.

Dr. J. A. FLEMING: It is very difficult to decide who ought to be congratulated the more heartily this evening; the members of this Institution for having had an opportunity of hearing such a delightful paper from Mr. Duddell, or Mr. Duddell himself for having completed such an excellent piece of scientific work. It is a paper which I am sure must especially gladden the heart of our President, for if I mistake not it will have important practical results in connection with electrical engineering, and it will perhaps assist in showing that science sometimes does go before practice, and not follow it. Dr. Fleming.

The matter that has most interested me has been the experiments with the continuous-current arc setting up oscillations in a circuit containing a condenser short-circuited on the arc, because I have been attempting something in that direction lately, only unfortunately I have concentrated all my attention on the alternating-current arc between metallic electrodes, whereas I ought to have directed attention to carbon electrodes. I am afraid it will be a matter of regret to me that I made that choice. It is, of course, a well-known fact that if an alternate-current arc between metallic balls is used, strong oscillations can be set up in a condenser circuit shunting the balls, especially if the arc is blown upon by a well-regulated current of air. But the effect with the continuous-current arc is exceedingly interesting, because it seems to depend on certain critical conditions as to the state of affairs when the arc is extinguished. If I mistake not, a rough explanation of it is something in this direction. Imagine the condenser and inductive circuit to be placed across the carbons when the arc is in operation. Then at that moment the arc is robbed of its current, and is extinguished. Then the potential rises, the condenser becomes charged, and it discharges itself through what remains of the conducting vapour, and then re-establishes the arc, and so sets up a periodic state of affairs —a sort of flutter is created in the arc which expresses itself in a musical note. But, as Mr. Duddell points out, it will not take place if cored carbons are used, and it will not take place if metallic electrodes are used. It seems to be perfectly clear from his experiments that the reason why it will not take place with a metallic electrode is because the arc vanishes too quickly. Perhaps the reason why it will not take place with the cored carbons is that the conducting vapour remains too long. In that case I should like to know whether the sound will be produced by cored carbons if blown upon by a gentle current of air.

Certainly one of the most practically important parts of Mr. Duddell's paper is the final section, in which he deals with the question of switch contacts, and, if I mistake not, that will cause some heart-searching to those who have been responsible for the manufacture of high-tension switches. Hitherto the idea has been in the minds of every one of us that what a high-tension switch ought to do is to have jaws which fly apart as quickly as possible, and break the arc as rapidly as possible. That, according to these experiments, is exactly what it ought not to do. I lately came across, in a technical journal, an elaborate description of a switch for high-tension purposes. It was to be made of non-arcing metals. I suppose Mr. Duddell will tell us that that is exactly how not to do it, and that what we want to do in constructing a switch of that

Dr. Fleming. kind is not to destroy the arc, but to encourage the arc, only it must be an arc which is under perfect control, and which you can whittle away by degrees, having it under perfect control until it is finally extinguished.

I recollect trying many experiments with carbon poles underneath the surface of water drawn apart in the endeavour to combat some of those very difficulties which Mr. Duddell alludes to at the end of his paper, when he speaks about the risks and dangers of interrupting alternating currents in concentric cables. I suppose there is no question that some of those effects have been due to the very things he has explained to us to-night.

There is one other point I should like to notice, viz., experiments with the induction coil. It is well known to every one who handles induction coils that although a coil will work well with a certain number of cells, taking, we will suppose, five amperes and working with ten volts, yet if you try to work that coil upon a 100-volt circuit, and put in a resistance to keep the current down to five amperes, it will not work at all. You get a very reduced secondary spark out of it. And the explanation is clear. When you have the higher voltage, as the contact points separate the arc is drawn out, and the decay of the magnetism of the core is therefore hindered, which is exactly what you do not want. Therefore the moral of these things is, that in the contact of an induction coil you must do exactly the opposite to that which ought to be done when constructing high-tension switches. I am sure all this portion of Mr. Duddell's paper will have valuable consequences in directing those responsible for the design of large switches to consider their ways and be wise.

Mr. Trotter. Mr. A. P. TROTTER (*communicated*): The fact that I have published so little about my researches on the rotatory phenomena of the direct-current arc, makes me reluctant to criticise those parts of Mr. Duddell's paper which cover the same ground, and if I did differ with him on a few isolated points, I do so without diminishing my esteem for the paper as it appears in print, and my admiration for the experiments with which it was so brilliantly illustrated.

The sensitiveness of the arc to small variations of the current was brought to my attention by the clicks which corresponded to the commutator sections of a little motor which I used to drive my stroboscopic discs. The motor was in shunt to part of an iron-wire resistance in series with the arc. I used about 20 amperes on a 100-volt circuit. The motor took about one ampere; I substituted an induction coil for the motor, and the hum or squeak of the contact maker was clearly reproduced by the arc. In both of these cases the main current was not interrupted, but a small change was made in a shunt. I used to work late at night, and did not attempt serious work until I knew by the sound that the Kensington Court dynamos had shut down, and that the supply was derived from the batteries.

I do not think that the model exhibited to show the rotation of the arc gave a good idea of the actual conditions. The crater is horizontal, with a good arc between vertical carbons. With a silent, fairly short arc the cupped surface appears to be uniformly luminous, but on gradually

Mr. Trottor.

increasing the current, and before the hum begins, photometrical examination shows the rotatory phenomenon. The bright spot is the "head" of the comma-like patch of luminosity. This spins round, but always within the crater, and it can never be "on the other side of the + carbon" as Mr. Duddell suggests, unless the arc be very long, and the + carbon rounded instead of being flat or cupped, and the arc very irregular. I think Mr. Duddell has rather exaggerated the extent of the rotation, perhaps by way of making the point more clear. But I agree with him generally that the periodic variation of the light is most probably due to the fact that any one portion of the crater supplies at one time a brilliant and at another time a less brilliant light to the oscillograph mirrors. Unless the whole of the light can be collected and dealt with, I do not think that the author is justified in saying that "the light" varies with the frequency of the hum. It is not unlikely, however, that the total light *does* vary; but this, on account of the variation of current rather than on the strength of the author's photographs.

In 1894 I observed the variation of the current by means of a telephone wound with thick wire carrying the whole current of 20 amperes. I made some attempt to measure the variation, as an alternating current, but I had not the means for doing so: this was one of the matters which I wished to investigate before publishing anything more. Other matters which I have regarded as indispensable features of an account of the humming arc are a set of photographs of the "white spot," the "comma," and the "butterfly," and an investigation of the relation between periodicity, of hum, current, and length of arc. The variation of current has been admirably measured by Mr. Duddell, but he has not, I think, thrown any light on the cause of the variation. We merely have his statement, that with a humming arc the current and the volts and the light vary with the frequency of the hum. I see no likelihood of being able to continue my research at present, and I should be glad to see Mr. Duddell or some equally competent investigator take it up where I left off. I think that I can account for the rotation, but I am quite unable to understand the variation of the current.

After a long study of enlarged images of silent, humming, and hissing arcs, the author's diagrams, Figs. 6 and 7, do not convince me that Mrs. Ayrton is justified in suggesting that "the hissing arc is also probably rotating." Examination of the images of hissing arcs with various kinds of stroboscopic discs has not disclosed either rotation or periodicity. The diagrams show exactly what might be expected from a phonograph reproduction of a hiss. The image of the crater of a hissing arc shows a patch or patches of brilliant luminosity jumping about: I can find no better expression. They may flicker with more or less regularity for a second or two, but with an ordinary hissing arc the behaviour is erratic and capricious. Fig. 6 shows three waves having a periodicity of about one hundredth of a second; if this were continued for an appreciable period it would give a hum, sounding the lower G of the bass clef, and in so far as any periodicity exists, the arc is a humming arc as well as a hissing arc. As in the case of the humming arc, the record of the oscillograph cannot be accepted as

Mr. Trotter. indicating the variation of the whole light unless the whole light is dealt with.

Of the remainder of the paper I will say nothing but to repeat my admiration, and of the parts to which I have referred I would say, without disparagement, that they serve to fill up chinks between the classical paper on the Hissing Arc and one on the Humming Arc, which has not yet been written. He who has time and opportunities for writing the latter has some fascinating work to do.

Mr. O'Gorman. Mr. M. O'GORMAN (*communicated*): Mr. Duddell, with admirable self-restraint, puts the suggestion of damage to concentric cables from the sudden interruption of an arc between metal contacts in the shape of a question. His suggestion is very probably right in certain cases, but I think these cases are at present rare, for several reasons.

1. The capacity needed to extinguish suddenly a metallic arc when the voltage was 200 was shown by his experiments to be considerable —as cable capacities go. (The entire eleven miles of the Ferranti Deptford main has only a capacity of 3·8 microfarads, and Mr. Duddell used 5 mfd.).

2. That capacity must be, so to speak, immediately available, not separated from the arc to which it is a shunt by either self-inductions or a resistance, one or both of which are almost always present in the armature of the dynamo, motor, &c., if not in the mains, through which the cable capacity acts on the arc of a switch.

3. The capacity is almost invariably shunted in low-tension systems by the non-inductive filaments of the lights which are not extinguished at the time of the operation of the switch under consideration, for the load on a central station of any magnitude such as would have a large capacity on its mains never falls to zero.

4. The leakage from main to main is almost invariably sufficient to allow a normal current of a few amperes to flow; thus the capacity shunting the arc is itself shunted by a small resistance, say 100 ohms, even supposing the arc to be struck when there are no non-inductive filaments connected.

5. It was proved by Northrup and Pierce (*Electrical World*, Nov. 6, 1897), in a paper quoted by Steinmetz, that the disruptive effect of high-frequency oscillations from a condenser and self-induction, or the peaky volt surgings from an induction coil, is much less than that of a sinusoid alternating voltage, on heavy insulating oils (which are the basis of the bulk of modern concentric cables). Hence 2,000 volts does not of itself always mean a very heavy puncturing effect applied to a cable, though it may mean a great deal with a piece of dry paper, the path across which is practically an air-gap.

On high-tension mains in ordinary practice we are far from getting the short snappy, almost explosive extinction of the spark which Mr. Duddell got on each occasion signalised by the paper puncturing, and I have been unable to speak to any one who has seen such a sudden interruption of the arc in practice on the mains. This is due to two facts. First, that a D.P. switch is always used which disconnects the condenser from the circuit while breaking it; and secondly, if one limb of the switch operates before the other has begun to arc, as is frequent,

or if a single-pole switch is used, the condenser is not directly in shunt across the metal arc between the contacts, but is across them, *in series with the self-inductions of the line and some part of the load in parallel with the alternator.* Whether alternating generators having small self-induction are for this reason to be preferred, is a question which is probably settled by the fact that all alternators now made have such resistance and self-induction that sparks are not usually swamped with this dangerous suddenness. Mr. O'Gorman.

Every one who heard Mr. Duddell's paper must agree that he may justly feel proud of his brilliant success, both in research work and in demonstration.

Dr. E. W. MARCHANT (*communicated*): There is a question which is of some interest in connection with one of the experiments Mr. Duddell showed at the demonstration on December 13th. I refer to that in which the music of the arc was first changed in note, then stopped altogether by the introduction of an iron-wire core into the self-induction he was using. Dr. Marchant.

Apart altogether from phenomena produced by energy absorption, the increased self-induction of the coil produces an increased impedance of the circuit, and a consequently decreased alternating current in the circuit, the impressed P.D. remaining constant. This reduced current gives rise to a diminished expansion and contraction of the vapour column, and consequently a note of less intensity is heard. At the same time, the absorption of energy in the core prevents the amplitude of the oscillations in the circuit increasing beyond a certain limit; they are damped out too quickly. I wish to draw attention more particularly to the causes of the energy absorption. Mr. Duddell describes the effect as being due to hysteresis, but I think he should describe it as an effect partly due to hysteresis and partly to eddy current loss. It does not seem to be generally recognised that the eddy current loss in iron is always very much greater than in any other metal, copper for instance. In several instances descriptions of effects produced by iron wires that are not produced by copper wires have been published, and the cause of the difference in behaviour of the two metals at once put down to hysteresis without taking account of eddy currents, quite forgetful, apparently, of the fact that eddy-current loss in iron wires at ordinary induction densities, is many thousands of times greater than in copper, in spite of the relatively great conductivity of copper. For example with No. 28 soft iron wires, with an induction density of about 15,000 lines per sq. cm. and a frequency of 1,000, the eddy-current loss is double that which is due to hysteresis. It would seem, therefore, in this experiment that eddy currents are at least as potent as hysteresis.

The very interesting experiment shown by Mr. Duddell with an induction coil, in which the spark length from the secondary was increased by connecting the condenser the instant after the spark was formed at the contact breaker, is capable, I think, of a comparatively simple explanation. The experiments preceding this proved that the P.D. induced between metal electrodes is higher when the condenser is suddenly switched on with the arc burning, than when the

Dr. Marchant.

current is suddenly switched on, the condenser being always in connection.

When a condenser is switched on between the terminals of a spark-gap, oscillations are set up in the condenser circuit (the frequency of the oscillations depending on the conditions of the circuit) which may enable the arc to restrike. If, however, the time at which the condenser is switched on be so regulated that the arc will just not restrike, the current through the coil is annulled with great suddenness, at a rate calculable from the conditions of the circuit. If, on the other hand, the condenser be applied from the beginning of the break, the arc will be able to restrike, and rapidly alternating currents will traverse the metal arc until the distance between the contact points has so far increased as to prevent this. In other words, the cause of the phenomenon is the fact that the rate of flow of current into a condenser, and the consequent rate of extinction of the current through the induction coil, has its maximum value when the condenser is first connected, a fact known from the solution of the equations determining the charge of a condenser.

I can only add my congratulations to Mr. Duddell on his admirable paper, and still more admirable experiments.

Mr. Russell.

Mr. ALEXANDER RUSSELL (*communicated*): For his wonderful discovery of a simple method of obtaining alternating currents of high frequency from the direct-current mains, Mr. Duddell deserves the grateful thanks of all electricians. In case any of those who did not hear Mr. Duddell may think that the method requires elaborate apparatus or careful tuning, the following account of a rough experiment with an ordinary direct-current arc lamp may prove instructive. The lamp was run direct from the hundred-volt street mains through a resistance, and had the ordinary shunt and series regulating coils. A coil of 110 yards of 7/15 cable about 2 feet in diameter, wrapped up as it came from the makers, was put in series with a condenser, and the two were placed as a shunt between the carbons. The condenser was a very roughly made one of about 1·9 microfarad capacity, and was similar to those used with ordinary induction coils. On switching on the current, which was about twelve amperes, a high musical note could be heard very occasionally, but on reducing the current the note became continuous. Placing another coil of cable in the neighbourhood of the first coil, it was easy to feel and easy to see by the sparking on breaking the circuit that powerful induction effects were taking place between the two coils. The induced E.M.F. could also be read on a hot-wire voltmeter up to a distance of two or three feet between the coils.

A Siemens' electro-dynamometer placed in the main circuit read 3 amperes (C), and another in the condenser circuit read 2·1 amperes (I). The resistance of the coil of cable and of the dynamometer and leads was about 0·25 ohms (R), and the musical note given out by the arc showed that the frequency of the alternating current was about two or three thousand per second. The P.D. between the carbons was 48 volts, and the current in the main circuit did not appreciably alter when the condenser circuit was switched on and off.

The power-factors of the arc and the condenser circuit can be easily found. Let $V + e$ be the P.D. between the carbons where V is a constant and e varies, then, as is well known, the effective P.D. will be $\sqrt{V^2 + v^2}$, where v is the effective value of e. Let also R be the resistance of the condenser circuit, shunting the arc when the condenser is short-circited, and let I be the effective value of the instantaneous current in this circuit, then the power expended in it will be $R I^2 + H$, where H represents the power expended in the condenser and in neighbouring metallic circuits. When we can neglect H, the power-factor of the condenser circuit Mr. Russell.

$$= \frac{\text{true watts}}{\text{apparent watts}}$$

$$= \frac{R I^2}{\sqrt{V^2 + v^2}\, I}$$

$$= \frac{R I}{\sqrt{V^2 + v^2}}$$

$$= 0{\cdot}011.$$

[On switching off the condenser circuit the P.D. fell from 48 to 40. Hence $\sqrt{V^2 + v^2} = 48$, $V = 40$

$\therefore v = 27$

In practice v and I do not remain steady for more than a few seconds at a time, and vary between wide limits.]

If C be the current in the main circuit, then the alternating component in C is very small, and so we can consider C as constant.

$$\left.\begin{array}{r}\text{The instantaneous power}\\ \text{expended in the arc}\end{array}\right\} \begin{array}{l}= (V + v)(C - i)\\ = V C - v i + v C - V i.\end{array}$$

Now the mean value of the last two terms for a complete period is zero, and the mean value of $- v i$ is $- R I^2$.

$$\therefore \text{Power expended in arc} = V C - R I^2$$

$$\therefore \text{Power-factor of the arc} = \frac{V C - R I^2}{\sqrt{V^2 + v^2}\sqrt{I^2 + C^2}}$$

$$= 0{\cdot}68.$$

The current through the arc is of course $\sqrt{I^2 + C^2}$, *i.e.*, $3{\cdot}66$ amperes. The power-factor of both arc and condenser circuit taken together

$$= \frac{V C - R I^2 + R I^2}{C \sqrt{V^2 + v^2}}$$

$$= \frac{V}{\sqrt{V^2 + v^2}}.$$

Hence, as Professor Ayrton pointed out, we have part of a direct-current circuit with a power-factor less than unity. The determination

Mr. Russell. of v is not very easy when v is small, as V and $\sqrt{V^2 + v^2}$ are nearly equal to one another. For example, if V is 48 and v is 6, then the difference between V and $\sqrt{V^2 + v^2}$ is only 0·37 of a volt.

Mr. Duddell mentions that for a very rapid rise of i, v shows an initial rapid rise, and hence v and i will have the same sign. This seems to indicate that when the frequency of the natural vibration of the condenser circuit is greater than 10,000 it may be impossible for its current to absorb sufficient energy from the arc to keep up the vibrations, and hence the phenomenon would cease. As Mr. Duddell has not yet published his experiments on the resistance of the electric arc, it perhaps would be hardly fair to ask him to elucidate more fully some of the results in connection with it he has mentioned. So far as interest and importance are concerned, the paper by Messrs. Duddell and Marchant on alternate current arcs was hard to beat, but I think Mr. Duddell has done it.

Mr. Clinton. Mr. W. C. CLINTON (*communicated*): The extinction of the metal arc when shunted by a condenser is probably materially assisted by the superior conductivity of metal electrodes. In confirmation of this it may be noted that extinction under given conditions is certain with copper poles, less certain with zinc, and does not take place with carbon. It would be interesting to know whether extinction is accomplished with the same certainty using copper after the arc had been running for, say, an hour, and everything was thoroughly hot.

Mr. Duddell. Mr. W. DUDDELL (in reply) [*communicated February* 22, 1901]: To the interesting remarks made by Professor Ayrton on the connection between my experiments and those of Messrs. Frith and Rodgers, and on the bearing of these experiments on the value of the resistance of the arc, I will make no reply, as I hope at an early date to enter very fully into this subject.

The fact that the power factor of a direct current hissing arc is less than unity is evident from Figs. 6 and 7, and its value could be calculated from them. Mr. Alexander Russell justly points out that the power factor of the Musical Arc is also less than unity; in fact, if an arc, or any other conductor which has a resistance or an E.M.F. depending on the strength of the current, forms part of a circuit through which a varying current flows, so that the instantaneous values of the P.D. and current do not have a constant ratio, then the power factor of that circuit will be less than unity.

Dr. Fleming, in his explanation of the phenomena of the Musical Arc, assumes that the arc is extinguished at each oscillation; but this is not necessarily the case, as by changing the conditions of the arc, the current can be caused, either to vary over a very limited range without at any instant becoming very small, or it may be caused to vary over such a large range that actual reversal of the direction of the current through the arc takes place, the arc current becoming practically an unsymmetrical alternating current.

Dr. Fleming also asks whether cored carbons can be used if the arc be blown upon by a gentle current of air. Intermittent arcs can certainly be obtained between cored carbons by this means; in fact, so far as I have tried, *intermittent* arcs can be produced between any elec-

trodes with suitable circuit conditions and blowing either by means of a magnet or a current of air. This latter phenomenon, where the arc is, so to say, mechanically extinguished and relights, must not be confounded with the Musical Arc, which depends for its action on a certain specific property of the arc considered simply as an electrical conductor, no actual extinctions or intermittances of the arc being necessary for the phenomena to maintain themselves continuously. Mr. Duddell.

Dr. Marchant raises the question of the connection between the value of the alternating current in the condenser circuit shunting the arc, and the frequency. I have measured this current, and I find it very little affected by change in frequency; this is probably due to the fact that the periodic time of the current is always the same as the periodic time of the circuit, so that it is the resistance of the shunt circuit which determines the flow of the current and not its self-induction or capacity. Dr. Marchant says that "eddy current loss in iron is many thousand times greater, at ordinary induction densities, than in copper," but is this a fact? If the *induction* is the same, then the loss in copper, other things being equal, is greater than in iron. Does not Dr. Marchant mean for the same *magnetising force?* Taking the case of the experiment shown at the meeting of an iron wire core introduced into the self-induction coil stopping the note. The coil used consisted of 98 turns of No. 12 D.C.C. wire, mean diameter 35 cms. Self-induction without core $5{\cdot}3 \times 10^{-3}$ henry. The core used consisted of a bundle 3 cms. diameter of No. 26 iron wires 54 cms. long. Weight 1·9 kilogrammes. The frequency was about 950 $\sim$ per second without the core and was reduced by its introduction, the arc just failing to emit its note when the core was central in the coil. It is improbable that the induction in the core attained 1,000 lines per cm.^{2} so that although the eddy current loss is considerable, I still think that the cessation of the note was mainly due to hysterises, though whether it was due to hysterises or eddy current losses does not affect the object of the experiment, viz., to show that causes which tend to dissipate the energy in the condenser circuit may stop the arc giving its note.

Mr. Clinton asks whether the extinction of the metal arc when shunted by a condenser will take place with the same certainty after the arc has been running for, say, an hour.

I have no experience of metal arcs which have been running for such a long time; after a few minutes the arc is burning between globules of molten metal, and in this condition the extinction still takes place. Any further burning will not, I think, materially affect the conditions, as the molten metal then drops off as it is formed. I am of the opinion that the suddenness does depend on the quickness with which the vapour condenses under the given conditions; thus, if the whole space in which the arc was burning was at a higher temperature than the temperature of volatilisation of the metal, I should expect that extinction would not be produced.

In connection with the subject of the dangers of capacity shunting the switch contacts in inductive circuits, Mr. O'Gorman advances several reasons for thinking that at present such dangers are rare. I am not, however, inclined to agree with him, and if I were re-writing

Mr. Duddell. my paper at the present time, I should no longer use such self-restraint as to put the suggestion of damage to concentric cables in the form of a question. I will consider the reasons he gives as far as possible in order.

1. There are in use at the present time, and their number is rapidly increasing, a considerable number of concentric cables having two or more microfarads capacity, and these capacities would be sufficient to produce the sudden extinction of the arc between the switch contacts under my conditions. It must also not be forgotten that the smaller the capacity, other things being equal, the higher will be the rise of P.D. between the terminals of the switch, supposing the arc is extinguished.

2 and 3. Mr. O'Gorman is quite correct that the capacity must directly shunt the switch contacts and not be separated from them by any considerable self-induction or resistance. This actually occurs in

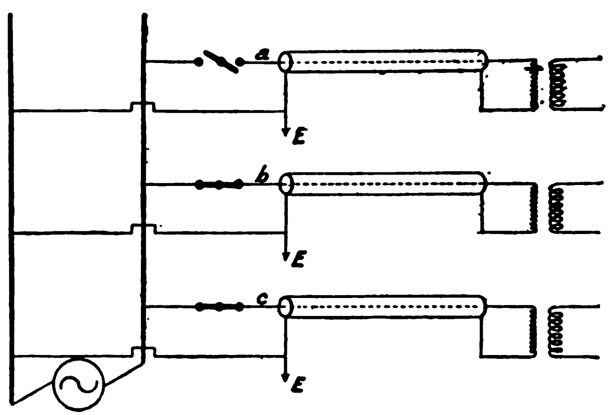


FIG. C.

practice in several cases. I will take as an example the ordinary case of switching off a high tension concentric cable from the buss bars, to which several such feeders are connected, as shown in Fig. C. The outers are all assumed connected together and to earth. The capacity of the inner of, say, cable *a* to the inners of cables *b*, *c*, &c., will be due to the capacity of *a* in series with the joint capacities of *b*, *c*, &c., and will have a value which will range between one half, in the case of two identical cables, and equal, in the case of a large number of cables, the capacity of the inner to the outer of each cable. This capacity of the inner of *a* to the inners of *b*, *c*, &c., *directly shunts the contacts of the switch* in the inner of *a*, so that if an inductive load be connected to the cable, we have all the necessary conditions for a serious rise in P.D. between the switch contacts and consequent danger to the cables.

In spite of Mr. O'Gorman's statement at the end of his remarks "that a D.P. switch is always used which disconnects the condenser from the circuit while breaking it," I think it is more usual in the case of high tension feeders to employ single-pole switches (Ferranti switch

gear), though even when D.P. switches are used it can easily be seen that they would not prevent some considerable capacity from still *directly shunting the switch contacts.* Another case in which capacity directly shunts the contacts is that of a switch in a substation which joins the inners of two concentric cables. The insulation resistance of high tension cables will in general be several megohms, so that there will not be sufficient leakage to reduce the danger. Mr. Duddell.

In low tension networks the dangers are much less; probably the worst are those due to a short circuit or to a large motor failing to start up, resulting in the action of some automatic cut-out or fuse whose contacts, or those of the short circuit itself, are practically directly shunted by part of the capacity of the system. If the capacity is shunted by a non-inductive resistance as low as 100 ohms I should certainly expect the danger to be very small.

5. Without further experiment I am unable to say what is the puncturing effect of the rises in P.D. I have observed, as I have always had to be most careful to keep the rises well under control to avoid breaking down the condensers. One thing is, however, quite certain, and that is that even with experiments on such a small scale as those described in the paper the puncturing effect is considerable.

There is one other point which I have not yet alluded to, and that is the question as to whether a distributed capacity such as that of a concentric cable will behave the same as a condenser. It was this doubt which led me to put my suggestion of the possible danger to cables in the form of a question. Owing to the courtesy of Mr. Minshall I have been able to test this point experimentally with an actual cable under more nearly practical conditions, and I find that the rises in P.D. do take place when using the distributed capacity of a concentric cable.

Mr. Trotter alludes to the extreme sensibility of the arc to small variations in the current through it. Since writing the paper I have heard of another rather interesting example of this sensibility. Whilst I was making experiments on the Musical Arc at the Central Technical College, obtaining my current from the street mains, Mr. W. Bradfield noticed that an arc, with which he was working in Sir W. de W. Abney's laboratory, and which was also supplied by the street mains, was playing a tune.

Thus this latter arc, which was burning under ordinary conditions and was not adjusted in any way to make it sensitive, detected the effect, on the distributing network of a large supply station, produced by my Musical Arc taking a current which was varying by about half an ampere from the mean, although the two arcs were in totally distinct buildings, at a distance, in a straight line, of about 400 yards from one another, and at a considerably greater distance if measured along the street mains.

Mr. Trotter is quite correct that the model of the humming arc shown at the meeting was greatly exaggerated, but this was necessary to make the effect visible to those at the back of the lecture hall. When I said that the light varies, I thought that I had made it clear that the light to which I referred was that given out in the direction of my lens,

Mr. Duddell. as I use at the end of my section on the humming arc the expresssion "the light emitted in a given direction." In order to make this quite clear I have inserted the words "in a given direction" into my conclusions at the end of the paper.

It is admitted, I think, that the humming arc rotates. Suppose it is once started in rotation, it cannot continue so without some force or forces are acting on it tending to maintain the rotation. The question is, what is the nature of these forces? At first sight there are two possible causes outside the arc itself which may tend to maintain the rotation and humming. I refer to convection currents of air and to the effect of a magnetic field. The first of these does not seem to be the true explanation, as the arc will hum in any position even with the carbons horizontal; the second, the magnetic field, is also for a similar reason excluded; in fact, the arc will still hum even if deflected to one side of the crater and kept there by means of a magnet. So that neither of these causes seems able to supply a satisfactory explanation of the rotation of the arc observed when humming.

Within the arc itself there is a possible cause for the rotation, viz., if the arc is burning between any two points, and there exist contiguous to them any two other points between which the current would do less work in maintaining the arc, then the arc will tend to move from between the former to between the latter points—that is, under ordinary conditions the arc will tend to move to between those points requiring the lower P.D. Suppose the arc is rotating, I will call the side of the spot where the current passes from the carbon to the gas, which *leads* in the direction of rotation, and which is constantly moving to points on the end of the carbon through which no current was passing, the *leading side*. In order that the arc may continue to rotate, it is necessary that the leading side should be moving into successive positions which require less P.D. to maintain the arc than do the other sides. There are three possible causes of such an effect. (1) The ends of the carbons may be nearer together at the leading side than elsewhere. (2) The oxygen of the air may obtain better access to the leading side, either directly or by being absorbed in the carbons. (3) The temperature gradient at the leading side may be different to elsewhere. Of these (1) does not seem to be the true cause of the rotation, as the arc will often move from burning between points at a shorter distance apart to between those at a longer; though a variation in distance in conjunction with the rotation of the arc may well be the cause of the observed variation in P.D. and current. (2) The direct access of the oxygen of the air to the hot carbon and its combination with it would seem to tend to stop the rotation, as it would be less likely to combine with the cooler carbon at the leading side and produce a drop in P.D. there, such as Mrs. Ayrton discovered in her experiments on hissing, than to combine with hotter carbon at the other sides. It is difficult to say whether the oxygen absorbed in the carbon would behave in the same way. (3) The possible less temperature gradient at the leading side seems to me one of the most probable causes of the rotation, though considerably more experimental evidence as to its effect on the P.D. required to maintain the arc is necessary before any very definite

opinion can be expressed. If the opportunity offers I hope to continue my experiments on humming along this line. Mr. Duddell.

In conclusion, I wish to express my thanks to the members of the Institution for the very kind way in which they received my paper. I wish also to thank Messrs. G. Wall, L. Murphy, R. M. Moberly, and J. F. Hunt for the untiring way in which they helped me to prepare and carry out the experiments shown at the meeting.

The President.

The PRESIDENT: I will now ask you to give a vote of thanks to Mr. Duddell for his paper. I really do think we have had the most extraordinary luck this session with regard to having good papers, and I think this is one of the best.

The vote was carried with acclamation.

The Three Hundred and Fifty-Fifth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, December 20th, 1900, Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on December 13th, 1900, were read and approved.

The names of new candidates for election into the Institution were announced, and it was ordered that they should be suspended.

The following transfer was announced as having been approved by the Council :—

From the class of Associate Members to that of Members—

Francis E. Procter.

Donations to the Library were announced as having been received since the last meeting from Mons. G. Eiffel, to the Building Fund from Messrs. J. S. Highfield, G. F. R. Jacomb-Hood, and R. H. Sperling, and to the Benevolent Fund from Mr. J. S. Highfield, to whom the thanks of the meeting were duly accorded.

Messrs. Hal Williams and Alan Williams were appointed scrutineers of the ballot for the election of new members.

The following paper was read :—

THE ELECTRICAL ENGINEERS R.E. IN SOUTH AFRICA.

By LIEUT.-COLONEL R. E. CROMPTON, E.E. (R.E.) V., Past-President.

At the request of your President I have prepared a narrative of the war services of the small body of electrical engineers who left England early in the spring of this year for South Africa and who have now returned.

I must commence by reminding you that the corps of Electrical Engineers R.E. Volunteers was raised by the help of this Institution in order to enable electrical engineers to place their services at the disposal of the country in case of need. It is acknowledged that every year electrical appliances will be more and more used in military operations. Up to last year the special work on which the corps had been instructed and drilled for home service has been the management of the searchlight apparatus used at various points for coast defence, to which has been added some instruction in submarine mining and cognate matters.

As both officers and men of the corps felt that the training which every one in the electrical profession must go through has made us all handy men, not only for electrical, but for general mechanical engineering work, we offered our services for the war in South Africa. Our offer was accepted in December, 1899, and after some consultation with the War Office an equipment was designed by our own officers and was manufactured with such rapidity that our first detachment was ready for embarkation early in March.

Five officers and forty-seven men were mobilised in February, and on the 16th of March five officers and forty-seven men embarked from Southampton on ss. *Tagus*. On the 17th two officers and one non-commissioned officer embarked on the *Custodian* in charge of the equipment. The equipment has already been described in the technical journals; I will remind you that it consisted of portable searchlights mounted on carriages somewhat similar to those used for field artillery guns, the electrical energy being generated by dynamos carried on small compound steam traction engines. Special cable carts were also provided.

I do not propose to take your time in giving the details of construction of the projectors, engines, dynamos, or other apparatus; those interested in these matters will have full opportunity of examining it in March next, when the headquarters of the corps are completed.

Although steam traction engines were chosen, it was foreseen that the oil engines to which the corps is so well accustomed at its annual trainings would have hac

[illegible]

After a journey of three days and four nights it arrived at [illegible] of April, [illegible] off-loaded the equipment, and within a few hours the first installation of temporary electric light for war purposes was erected on the [illegible] road bridge, which was then temporarily in use for railway purposes, during the time that the low-level deviation railway bridge was then constructed by the Railway

FIG. 1.—E. E.-pattern Portable Searchlight with Drag-ropes.

FIG. 2.—Train showing Engine on Truck.

Pioneer Regiment to replace the original railway bridge which had been destroyed by the Boers.

At first six arc lamps were found sufficient, and these were erected and worked nightly from one of the dynamos mounted on its traction engine at the river-bank. As soon as the low-level railway bridge was completed the installation on the road bridge was dismantled and transferred to the new bridge, the approaches to which, being tortuous and having steep gradients, had to be worked by the engine drivers with the greatest caution, so that for some days the arc lights were used to light these approaches.

The first time the field telephones were put to work was at this place. A line was laid across the bridge for the use of the railway, and a good telephone service established.

During the time that the corps was at Bethulie Sergeant-Major Brown, R.E., and Sapper Phillips, E.E., were sent out with part of the telephone equipment to accompany a flying column which left Bethulie on May 2nd to join General Hart, who was then moving south from Smithfield.

As the telegraph line between Bethulie and Smithfield had been destroyed the new field telephones were brought into use to maintain communication with the flying column, and this was kept up without any failure during the time the column advanced until it met General Hart. General Hart was thus enabled to be put into communication with Lord Roberts *viâ* Bethulie, temporary communication being made between General Hart's camp, one and half miles from the telegraph line, by bare copper wire No. 22 gauge laid on the ground from the bicycle reels supported by a special belt to the body.

On the 6th of May the Unit arrived at Bloemfontein and was split up into two sections, one going on direct to Rail-head, which was then at Vet River, the other section remaining at Bloemfontein to carry out the electric lighting of the goods yard and locomotive shops. At this point an installation of arc and incandescent lamps was erected and worked by No. 1 engine and dynamo, and has been running ever since, the installation being gradually increased in size until there are now sixteen arc lamps and a large number of incandescent lamps installed. It must be noted that it is somewhat of a feat to carry out a temporary job of railway lighting of this kind, and to work it continuous'

without any failure or interruption from May to December (*i.e.*, up to the date of writing), using only one engine with no spare plant, and the credit of this excellent service must be mainly given to the small detachment that was left at Bloemfontein under command of Corporal Bicker Caarten, to whom has since been entrusted the design for a permanent installation to replace this temporary plant.

The advance section at Vet River joined the reconstruction trains and remained with them during the months of May and June—*i.e.*, until the work of reconstructing the railway was completed through the Orange Free State up to the Vaal River.

The equipment of this No. 1 section consisted of No. 2 traction engine with its dynamo, two waggons, subsequently increased to four waggons, two projectors with their limbers, two cable carts, extra cable coiled on drums, sixteen arc lamps with suitable poles, hoisting gear and the necessary accessories, a supply of incandescent lamps with suitable fittings, fourteen sets of field telephones, a supply of insulated and bare copper wire telephone conductors coiled on reels which could be either carried on the bicycle or on special slings strapped to a man's chest in such a position that he could either pay out or wind up a conductor.

The work at Vet River may be taken as typical of many other works at which it was necessary to work all night by artificial light. The Railway Construction Corps R.E. was engaged in constructing a main bridge across the river approached by long deviations from the main line; in these were three smaller bridges. The lighting plant had therefore to be erected at each of these bridges in turn.

When the Vet River work was completed the section moved on to Smaldeel, and there was joined by me with a draft which had left Cape Town on the 4th of May. I brought with me a third traction engine with its dynamo, an additional supply of concentric cable, two additional 5-ton waggons, stores, bicycles, &c., and then took over command of the Unit.

As occasion arose, the whole of the Bloemfontein section with their stores was gradually moved up to join the headquarters of the Unit, leaving only a small detachment of six men under the command of Corporal Bicker Caarten, vork the temporary lighting plant at Bloemfontein.

FIG. 3.—Destroyed Bridge (Zand River) as left by Boers.

FIG. 4.—Rhenoster Bridge, showing Girder down, and Arc-lighting.

the enemy, the Electrical Engineers restored the communications so quickly that practically no delay occurred in transmission.

We should also mention that the telegraph instruments found in the Transvaal were all of the "closed circuit" pattern (requiring the key to be lifted up instead of pressed down, as in the English pattern). The Electrical Engineers converted them to the open circuit—not an easy matter for men only provided with screwdrivers, knives and pliers of their belt equipments.

Captain Bain, who was eventually assisted by Lieutenant O'Shaugnessy, remained at this work for several months and eventually was put in charge of the whole of the railway telegraphs in the Transvaal under Captain Manifold, R.E., Administrator of Railway Telegraphs, his detachment being strengthened from time to time by additions from this Unit, as well as from the Royal Engineers and other corps.

The No. 1 section commenced lighting work at Zand River on the 18th of May, this work being of a very similar nature to that of the Vet River. It afterwards advanced with the Railhead Construction trains to the Rhenoster Spruit to assist the working party building a crib bridge at that point. Here a particularly smart piece of lighting work was carried out. The orders were received at 2 p.m. The traction engine was off-loaded from its truck and hauled its waggons and stores down to Rhenoster Spruit three miles away, the light stores going down the damaged railway line on a trolley. Eight arc lamps were fixed on poles and light was furnished to the working party by nightfall, 5.30 p.m.

The new and first reconstructed bridge at Rhenoster was completed on the 29th, and there being then no large work ahead requiring arc lights, the detachment went forward with the construction train and were employed principally on the actual work of building the crib piers, fixing the beams and laying the rails until the line was completed and railhead advanced to Taibosch. During this time the equipment was left behind at Roodeval in charge of Sergeant Phillips.

During the next eight days the detachment had an exciting time, as the enemy under De Wet and Theron were threatening the line and reconstruction works. The electrical engineers formed part of the reconstruction day

shift, the working hours being generally from 6 a.m. to 6 p.m., but sometimes up to 12 midnight, so that on these days the men were eighteen hours consecutively at work.

From Rhenoster up to Taibosch the enemy at the advice of Theron had destroyed the line very completely, not only the larger bridges, but all the smaller culverts were blown up; in some places the rails for considerable distances were twisted and contorted by dynamite cartridges fired at every fish joint.

On Wednesday, 6th of June, Taibosch Spruit was reached, and seeing that arc lights would again be required at this point and at the Vaal River, Captain Lloyd was sent back with two engines and a few trucks to bring forward the electric light equipment which had been left at Roodeval. His journey down the threatened line to Roodeval was a most exciting one. At Vredefort Weg he was called upon to arrest the Field Cornet Le Roux, who, although he had taken the oath of allegiance, had been observed signalling to the enemy. This train was expected to be attacked at any point south of this, and the utmost precautions had to be taken. As the water supply at Roodeval was known to be scanty he left one locomotive to fill up its tanks at Rhenoster and took one on to Roodeval. He there found that the small garrison had been threatened for days, and had been on harassing night duty during that time.

On his approach to Rhenoster Spruit he found that the line was blocked by some trucks which were being used, somewhat irregularly, for shifting tents and camp equipment required for a new camp.

As Captain Lloyd knew that De Wet was preparing to cut the line to the north of Rhenoster this delay seemed likely to result in the Unit losing their electrical equipment. As it was the line was only cleared in time to get the train away a few hours before De Wet commenced his attack on the Rhenoster camp, which resulted in heavy losses and in the destruction of the new bridge, the water supply, and of the valuable ammunition and stores then at Roodeval.

The return journey northwards was of the most exciting nature. The train was made up with some of the trucks in front of the locomotive, so that in case of the leading truck being derailed it could be thrown off more easily and thus less delay caused than if the locomotive itself were derailed.

It was necessary to examine the track with lamps placed on the leading truck, and thus by lamp signalling back to the engine driver to control the running of the train. In this manner the train was worked back to Taibosch.

At Vredefort Weg Captain Lloyd arrested and brought along with him the Field Cornet Le Roux. By the time Captain Lloyd passed Vredefort Weg De Wet had been successful in his attack at Rhenoster and cut both railway and telegraph wires, so that the construction trains were entirely cut off from the south.

During the time that the arc lights were being used on the bridge at Taibosch on the nights of the 7th and 8th Lieutenant Pott succumbed to enteric fever. This was a great loss to the Unit, as this officer had himself designed a great portion of the arc lighting and projector plant, and the loss of his services greatly crippled the Unit. He had to be left at Viljeonsdrift, where he was nursed with extreme care by Dr. and Mrs. Dixon, Dr. Dixon being the medical officer to this section of the railway.

Here Sapper Weakey also fell ill of enteric fever, probably brought on by the exposure at Roodeval. He lingered for some time, and eventually died at Viljeonsdrift on the 27th.

During the time that the Unit was at Taibosch Major Crompton, taking with him Sergeant-Major Brown, R.E., and Companys Sergeant-Major Rorke, went south with the field telephones to attempt to make temporary communication at the point where the telegraph lines had been cut by the Boers. Major Crompton returned to his command at Taibosch, leaving the two non-coms. with Lord Kitchener, who arrived at Kopjes on the 9th. They were able to establish communication with the south, and thus kept him in touch to north and south until the telegraph lines were again in working order.

Railhead reached the Vaal River on the 10th of June, and at this point an installation of twelve lamps was erected, and on the 11th the first train crossed into the Transvaal and some supplies were sent to Lord Roberts at Pretoria.

It may be here remarked that as during the advance of railhead from Bethulie to the Vaal the arc lamps were worked on nineteen nights, it is probable that the period of reconstructing the line was shortened by nearly the same

number of days, but even if one night's work is only considered to be equal to two-thirds of a day's work the night work rendered possible by the section must have hastened the advance of railhead by not less than twelve days, and the money value alone of this to the nation must have many times repaid the entire cost of equipping and sending out the Electrical Engineers.

After one night's lighting at the Vaal the very thorough destruction of the line to the south by De Wet necessitated the sending south for a second time both construction trains; the Unit accompanied them, leaving the lighting equipment at Viljeonsdrift under Sergeant Brown.

The construction trains working south were constantly threatened by De Wet, and on the night of the 14th of June both trains were attacked by him, and were summoned to surrender.

As this was the first occasion on which the Electrical Engineers were under fire, perhaps I may be permitted to describe the attack at some length :—

Very early in the morning of the 14th of June the night working party, which consisted chiefly of Royal Engineers and volunteer Royal Engineers, none of them being Electrical Engineers, were at work rebuilding the crib piers of the recently constructed bridge at Leuwspruit; at this time one of the construction trains was drawn up close to the working party, who were at the break in the line, the other train being about half a mile to the north on its way to shunt empty trucks on a siding a mile further north.

The first trouble noticed was the derailing of the leading truck of the northern train. This was caused by a stone having been wedged by the Boers between the main and the guard rails. As soon as the train was pulled up it was fired on by an attacking party which apparently surrounded it. The troops on the train promptly replied, and with good effect, as the Boers drew off after half an hour's firing and concentrated their attack on the other party at the Spruit, but during this sharp attack the vans and coaches of the northern train were riddled by bullets, and those sleeping in them had narrow escapes. The attack coming at first from the south side, the engine driver started to move the engine northwards. The Boers then made a rush on the unarmed working party, who, with one officer and sixty

men were taken prisoners; the remainder, however, escaped, but could not rejoin the party defending the trains.

The Electrical Engineers had formed the day-working party, and having been many hours at work, and consequently very tired, were sound asleep. Those of them who were sleeping outside the trucks, close to the working party, were captured by the Boers before they were able to regain the train. At this time the Boers' firing was very heavy, and three officers, *i.e.*, Lieutenant Micklem, R.E., Lieutenant Bigge, E.E., and Lieutenant Holmes, of the Royal Irish, were seriously wounded. Three sappers were killed, the engine driver and a number of others seriously wounded, and of the native workmen about thirty were killed or wounded. At this time De Wet sent in a messenger summoning the train to surrender as the Boers were greatly superior in force and had guns. No attention was paid to the message, and the remaining troops, who were then under the command of the officers of the Electrical Engineers, took up a position on a ridge of rocks which ran transversely to the train, from which the train could be protected without the men being exposed. This position was held for several hours—in fact, up to daybreak—the firing of the Boers being continuous and severe. On the other hand, the position we occupied was so strong that the Boers were unable to approach the train, and soon after daybreak they withdrew from the attack.

Towards the latter part of the time the Boers were much disconcerted by the shrapnel which was fired over them by two guns placed by Lord Kitchener in a position to the south.

The night was intensely cold and trying to every one; the whole of the defending party were asleep in the trucks when the attack commenced. Although taken at such a serious disadvantage, every man on the train, without distinction, behaved as coolly and quietly as if there were no enemy present.

It was a difficult matter even for old and trained soldiers to free themselves from the crowd of Basutos who formed the native working party, and who cleared out from the train at the commencement of the firing, but, in spite of this, there was no confusion; ammunition was quietly served out, and our men got into such strong positions that it was impossible

FIG. 6.—Scene of Action at Leeuwspruit.

FIG. 7.—Wounded Locomotive.

FIG. 8.—Firing from Van.

FIG. 9.—E. E.-Camp at Pretoria.

FIG. 10.—Park of Engines.

for the Boers to make their attack successful. Every one behaved so well that it is difficult to single out any one present for special praise. Although the Electrical Engineers engaged on this occasion were comparatively a small party, yet, owing to so many of the night-working party having been surprised and dispersed in the first instance, they formed nearly one-third of the troops left to defend the trains. If De Wet had been successful in catching and destroying these two trains, which contained so much construction material and all the tools, as well as the most skilled body of reconstruction officers, the break in the line of communications would have remained open for a long period, and certainly would have seriously affected the future movements of the more advanced portion of the Army then in the Transvaal.

This attack caused the Electrical Engineers to temporarily lose the services of one officer wounded, and Corporal Sellon and six Sappers taken prisoners. The remainder went south with the construction trains to Rhenoster, and assisted in the building of this important bridge. Whilst there, orders were received from Army Headquarters, that the whole of the Electrical Engineers should come up to Pretoria at once. They started on the 20th, leaving Lieutenant O'Shaughnessy to complete a system of telephones connecting Rhenoster Camp, Leuwspruit, Roodeval, and other points which had been recently threatened or attacked by De Wet. After carrying out this work he joined the headquarters at Pretoria on the 26th of June.

After arriving at Pretoria, the Engineer-in-Chief found quantities of electrical work for the corps, as all the forts which defend the town had been fitted up with various electrical apparatus, not only for electric lighting but there were signal signalling, telephone and other wires, which required to be sorted, labelled, and their uses understood.

The oil engines which supplied the motive power for driving the dynamos had purposely been damaged by the Boers, and had to be repaired and set to work. The small and inefficient searchlights which had been used by the Boers were replaced by a system of incandescent lamps placed so as to illuminate certain points which were liable to be threatened by the enemy during night attacks.

This work at the forts, which was of a very interesting

nature, was followed by teaching the garrisons how to use and maintain the electrical apparatus in working order.

Other parties of our men were employed in fitting up electrical lighting in various public buildings which served as supply stores, and in the large hospitals which were then being started, inside and outside Pretoria. In some cases these new installations were worked from the public supply of the town, in other cases separate generating machinery, *i.e.*, steam engines, dynamos, and systems of conductors had to be put down for each job. The plant for this was made up from material found at or near Pretoria, most of which was in a damaged condition and had to be almost re-made before it could be usefully employed.

During this time, as the whole of the Rand district surrounding Johannesburg is supplied with electric light and power partly from a distant generating centre at Brak Pan, the Military Governor of Johannesburg asked for and obtained the services of Captain Leaf as his electrical adviser, and from time to time various works were supervised by him and by other officers of the Corps, so that the electrical plant of Johannesburg and district was gradually got into order.

It will be interesting to those who have read Rider Haggard's novel, "Jess," to know that the camp at Pretoria was close to the site of Jess's cottage.

Its position was well chosen; a stream of water was handy for washing out the engines and it was close to the railway and railway workshops. Thus, it was in a most convenient position for repairing the various pieces of apparatus that they were installing in the forts and public buildings. Amongst others, the Government Printing Works of Pretoria, worked by their own electric generating plant transmitting power to motors driving each set of printing machinery, was put in order and worked by the Electrical Engineers.

Early in July, Electrical Engineers were several times called upon to extricate an engine which it was proposed to use to haul 12-ton 6-in. quick-firing guns to various positions near Pretoria. This engine and a gun were hauled out of a drift almost in the middle of Pretoria, into which it had sunk in the course of an experimental run.

A few days later, after the driver of this engine had placed

the gun in position near Deerdepoort, he took his engine down to a spruit in order to fill up his water tanks, but approached too closely to the soft ground, so that the engine turned over on its side and could not be got out until we were sent for.

Great praise is due to Captain Lloyd and Captain Leaf and the detachment engaged on this work, which was carried on throughout the night. The way in which the Electrical Engineers handled the engines and gun made it evident to the officer commanding the artillery that the Electrical Engineers were so capable, that he placed the engine and the two guns in their charge, and the subsequent work of placing these, the heaviest guns ever taken into the field, was carried out by the officers and men belonging to the Corps.

On the 24th of July, a 6-in. quick-firing gun, weighing with its carriage 12½ tons, was hauled to the top of Quagga Kop, seven miles to the west of Pretoria, and 1,300 feet above it; the average slope up which the gun was hauled was 1 in 10, but there were point parts of it which were steeper.

On the 1st of August a similar gun was hauled up a slope averaging 1 in 6, and in some places 1 in 5, to a redoubt on the top of this hill, about five miles east of the town. A few days later two large traction engines belonging to the Director of Steam Transport were put under our charge, under the officers of the Electrical Engineers, and with these and the other three engines, making five traction engines, a regular daily service was organised, and stores of every description were transported to various points, chiefly to the westward of Pretoria, the longest run being to Commando Nek, twenty-six miles distant, where a depôt was formed for flying columns. The service on this road lasted for many weeks, in fact, up to the time the Corps started for England, and was carried out without any mishap or loss, although during the whole of the time the line was threatened by the Boers, and we were informed that sniping frequently went on, but as far as we were concerned no one was hit. A new system of escorts was adopted; sufficient men to form two or more escorts being put under the command of the officers of the Corps and encamped with us.

The anxieties connected with this service were greatly reduced by an excellent system of signalling the position of the trains, which was introduced by Captain Lloyd.

Early in June a part of the detachment left at Bloemfontein left under Lieutenant Stubbs and worked under the Army Telegraphs repairing telegraphs and erecting new lines. They trekked the whole journey from Bloemfontein to Pretoria, only arriving at Pretoria towards the end of July. After their arrival at Pretoria Lieutenant Stubbs' party were employed for some time on new telephone lines connecting the forts with the town, and later on took another party down westwards to Standerton.

Towards the latter part of August a party of Electrical Engineers, under Captain Lloyd, were sent up to Brugspruit with a set of searchlight apparatus and accessories, which was erected, and the light shown from the top of the shaft of a colliery near Brugspruit Station, the dynamo being driven from the mine engine. Captain Lloyd returned to Pretoria and left the detachment for some weeks under the command of Lieutenant Bigge, and there is no doubt that the presence of this searchlight greatly reduced the liability of night attacks from a strong commando under Erasmus, which threatened the Pretoria and Delagoa Bay Railway at this point.

On the 4th of September one of the engines, with dynamo and sixteen arc lights, was taken down to Machadodorp by Captain Leaf and a small party of Electrical Engineers, and at this point an installation was erected to supply light to the various running-sheds and loading-stages, as at that time convoys which supplied three columns with food had here to be loaded.

On the 31st of August, Sergt.-Major Brown, R.E., and Corporal Dalton were attached to Lord Kitchener's staff to use their bicycles and field telephones as required at the front.

Up to the time of the arrival at Pretoria, as the officers and sappers were constantly employed on railway work, little opportunity was given for the sappers to use the bicycles, but after arrival at Pretoria they proved to be of the greatest value; for as the work at the forts and buildings was scattered over a large area it necessitated the officers and men moving rapidly from point to point, and

FIG. 11.—"No. 3" Hauling 12-ton Gun.

FIG. 12.—Heliographing for Orders.

FIG. 13.—Telephone worked on Bicycle.

FIG. 14.—Sapper Melsom with 90 lbs. of Wire on Bicycle.

the whole of the bicycles were in full use, so by their aid the twenty men who rode them were almost doubled in value.

On the 17th of September the writer, accompanied by Captain Leaf, was sent down by Lord Roberts to the low country to the east of Machadodorp, in order to report whether any of the traction engines could be efficiently used there. At this time Captain Bain also came down to Waterfal Onder in charge of the railway telephone work, and Lieut. O'Shaughnessy was in command of the more advanced telegraph work near Koomati Poort.

Early in October the writer was ordered to England by Lord Roberts, in order that his recent experience might be utilised in designing traction engines for military purposes. Captain Lloyd re-assumed command.

From this date the work went on uninterruptedly until the later end of October.

The unit by this time was in a very scattered condition, the large number of jobs being carried out all over the Transvaal and Orange River Colonies demanding small detachments at various places.

It was whilst in charge of his Standerton detachment that Lieutenant Stubbs so nearly lost his life. His duties necessitated a journey by rail on a line known to be threatened by the enemy.

At a point on the line near Vlaklaagte, a few miles north of Standerton, something was observed on the line which looked rather like dynamite.

Lieutenant Stubbs went forward to examine, and was immediately fired on by a large party of Boers concealed about three hundred feet off.

Trying to regain the engine he was hit again and again until at last he rolled into the ditch by the side of the line.

The train was captured and all not killed or wounded were taken prisoners.

The train was then blown up and burnt.

Towards the end of October orders were received from the Engineer-in-chief to concentrate the unit and hand over the majority of the stores so as to be ready to move homewards on receipt of further orders.

The various detachments were gradually collected, and on October 25th, exactly four months after they arrived in Pretoria, the unit steamed south for Bloemfontein.

Seven men were left behind in Pretoria to take their discharge there; four of these had been given civilian employment under the Imperial Military Government, and three were taken up by private employment on the Rand.

A forty-eight hours' journey took them to Bloemfontein, where they completed the handing over of equipment and packing for our final departure.

But even during this time the "handy man" was not allowed to be idle.

Six to eight miles of telephone line connecting outlying camps were laid under Captain Bain's direction, whilst Captain Lloyd was sent off to Norvals Pont to report on an electric-light installation at the hospital there. He also had to report on the future lighting of the railway depôt at Bloemfontein from the new Town Central Station.

Had not orders for the unit to move south finally for Cape Town been received it is probable that the men would still be discovering fresh jobs to turn their hands to.

Yet one more check was met ere the unit finally arrived at Cape Town.

No transport was absolutely ready, and so, to avoid the possibility of the hardened and fierce warrior striking terror into the hearts of the peaceful inhabitants of Cape Town, the bloodthirsty Electrical Engineers were kept for three days in the safe camp of Stellenbosch, and not until Saturday, November 17th, did they reach the port, where the train took them right alongside the *Norham Castle*. After an uneventful and extraordinarily fine passage the ship was brought alongside the quay at Southampton on the evening of the 6th of December.

ADDENDUM.

NAMES OF THE SOUTH AFRICAN DETACHMENT OF ELECTRICAL ENGINEERS (R.E.) VOLUNTEERS.

Returned to England.

Lieut-Colonel R. E. Crompton.
Captain F. L. Lloyd, R.E.

Captain A. Bain.	Captain H. M. Leaf.
Lieutenant J. J. F. O'Shaughnessy.	Lieutenant A. H. Pott.
Second-Lieutenant H. F. Bigge.	Sec.-Lieut. F. Powell Williams.

Sergeant-Major G. A. BROWN, R.E.
Company Sergt.-Major E. RORKE.
Sergeant W. S. ENTWISTLE.
Corporal H. H. BICKER-CAARTEN.
Corporal W. DALTON.
Second-Corporal E. M. SELLON.
Second-Corporal A. I. HODGSON.
Lance-Corporal H. R. ALLEN.
Lance-Corporal S. W. MELSOM.
Lance-Corporal C. F. LOVE.
Lance-Corporal A. R. PEART.
Sapper F. D. ARUNDEL.
Sapper W. C. COOKE.
Sapper A. J. C. DEVONSHIRE.
Sapper G. J. H. ELLIOTT.
Sapper E. C. HORSLEY.
Sapper A. E. MINNS.
Sapper N. W. PIRRIE.
Sapper F. G. PAYNE.
Sapper H. B. TILLEY.
Sapper F. J. WALLIS.
Sergeant-Inst. C. T. RUSE, R.E.
Sergeant J. H. S. PHILLIPS.
Sergeant A. H. I. GRAHAM.
Corporal A. CHARLTON.
Corporal W. H. HOSSACK.
Second-Corporal J. ROOK.
Second-Corporal C. LANDON.
Lance-Corporal G. CHARLTON.
Lance-Corporal D. F. COLSON.
Lance-Corporal M. N. CRAWFORD.
Lance-Corporal A. O. WILTSHIRE.
Sapper H. BERTRAM.
Sapper W. G. CARTER.
Sapper R. H. BRANDER.
Sapper R. W. HOLLIDAY.
Sapper L. H. HOUNSFIELD.
Sapper H. A. PARIS.
Sapper F. J. PHILLIPS.
Sapper R. B. ROBERTS.
Sapper J. D. TAYLOR.
Sapper E. J. WRIGHT.
Sapper F. J. YOUNG.

Accepted Commissions in Royal Field Artillery.

Second-Corporal D. G. W. HUME.
Second-Corporal J. H. STONE.

Remaining in South Africa.

Sergeant T. H. BROWN.
Sapper J. M. BOWREY.
Sapper C. W. S. PAINE.
Lance-Corporal A. E. LEVIN.
Sapper H. R. CLAXTON.
Sapper C. E SILVERTHORNE.
Sapper F. C. STEPHENS.

Left in Hospital in South Africa; but now returned to England.

Lieutenant J. H. STUBBS.
Lance-Corporal C. R. H. THORN.

Died in South Africa.

Second-Corporal A. HOLDAWAY (at Bloemfontein).
Sapper E. C. SHORT (at Kroonstad).
Sapper W. C. WEAKLEY (at Viljoen's Drift).
Sapper E. J. WEST (at Wynburg).

The PRESIDENT: We are not going to have any criticism on this paper. I think perhaps there has been a little too much criticism in this country of the military operations in South Africa. In addition to that, the only condition on which Colonel Crompton was allowed by the War Office to give us the paper was that it should not be criticised. For

that reason he has called it a lecture, and in calling it a lecture it is evident that he thought he could take liberties. When a man reads a paper he is supposed to read it as it is printed, but Colonel Crompton has added a great deal to the printed matter previously supplied. I very much hope that all the additions he has made will be inserted, as they are most interesting. I think we should also like Colonel Crompton to select a few of the views he has shown us this evening, in order that they may appear in the Journal. There were so many interesting things in the lecture that one does not like to particularise, but I am sure that the Norfolk Regiment, who, the lecturer said, were so easily educated in electrical matters, would value the testimony which Colonel Crompton had given them almost as much as any other testimony they might get during the war, however valuable. Again, without attempting to particularise, I may tell you that I have heard five different accounts of the Leeuwspruit fight from students of mine who were in it. From Colonel Crompton himself I have heard three different accounts, to say nothing of the account I got from Captain Lloyd. The account given in the paper is not like any of the others I have been told—it is too sober; from what I can make out it was the liveliest time on record for all our people. I did not know when I gave that address on Tuesday night what a lot of work had been done by the Electrical Engineer Unit in Africa, and yet I think I said on Tuesday night all I have to say, and I believe that my words voiced the feelings of the Institution. I have to announce that the Council has decided to issue to each member of the Electrical Engineers Unit a copy of that short address[1]

[1] ADDRESS OF PROFESSOR PERRY, AS PRESIDENT OF THE INSTITUTION OF ELECTRICAL ENGINEERS, TO THE SOUTH AFRICAN DETACHMENT OF THE ELECTRICAL ENGINEERS (R.E.) VOLUNTEERS, 18TH DECEMBER, 1900.

"Lieut.-Colonel Crompton, Captain Lloyd, Officers and Men of the Electrical Engineer Unit of the South African Army; in the name of the Institution of Electrical Engineers, I welcome you home.

"When Dr. John Hopkinson suggested that all British Professional men, and particularly Engineers, should prepare themselves in time of Peace for giving their professional services to their Country in time of War, he created the germ of what may become the greatest and best line of defence of the Empire.

on vellum paper, with the seal of the Institution. I will, now ask you to thank Colonel Crompton for his lecture with acclamation.

The vote was carried with acclamation.

Mr. ALEXANDER SIEMENS: I have great pleasure in proposing the following resolution :—

"That the Institution of Electrical Engineers offers the most cordial congratulations to the members of the Corps of Electrical Engineers (Royal Engineers) Volunteers, forming the Electrical Engineer Unit of the South African Army, upon the patriotic devotion that they have shown in placing themselves and their technical skill at the service of their country, and upon the successful termination of their arduous and self-denying labours during nine months of active service in the field."

No words of mine can support this resolution better than the lecture which we have just heard. I want to call attention to the skill which the officers leading the detach-

"You have carried out successfully the very first experiment. We of the Institution know how much you have sacrificed. I speak neither of social pleasures nor of home comforts. I do not speak of the dangers you have risked, either from the chances of war or from that more dreadful enemy, disease. These were common to you and all other Volunteers. But you gave up positions and chances of promotion in your profession, a sacrifice which we of the Institution are particularly well able to appreciate.

"For those who have died we mourn not 'as those without hope': Hope that a life laid down by a man for men is not a life thrown away; Hope for a country that can still breed sons who for her sake are not afraid to die.

"We are assured that in the opinions of the Generals your small Force was of very great service. In particular with the aid of your electric lights the work of mending the broken bridges went on by night as well as by day, thus saving this country millions of money, and who knows how many lives?

"You did any kind of engineering work that had to be done, and showed yourselves the handiest of 'handy men.' Officers and men alike took their full share of actual physical toil; but to us civilians, members of the Institution, the distinction between officers and men is merged in the fact that all alike were worthy representatives of the Electrical Engineering Profession. We stand higher in our own opinion and, as we believe, in that of the world, in that our profession has developed in you that common sense, that resourcefulness in time of trouble, that reserved power and moral courage which have distinguished you particularly in an army of distinguished men."

ments have shown in preserving their men from disease and from attack. In this you will allow me, perhaps, to say I am in a small way an expert, as I went through the same sort of experience thirty years ago, and therefore appreciate perhaps more than any of you the skill and the discipline which are necessary to produce the good results which Colonel Crompton has described to us to-night. It is also perhaps not a criticism of the paper if I add that the action of the Electrical Engineers, and their great utility during the war, has shown that the Volunteers are not such a despicable force as the enemies of volunteer service generally want to make out. Of course I served under conscription, but even then I can add my testimony that the best soldiers were the so-called Volunteers, the well-educated ones, because they did what the Electrical Volunteers have done—they obeyed orders, and did not talk. I have great pleasure in moving this resolution.

Mr. HUGO HIRST: It is indeed a great honour and privilege to be allowed to second Mr. Siemens' words of appreciation of the deeds of the Electrical Engineer Volunteers. May I add, at the request and with the permission of the President, a few words that suggest themselves to me on this occasion. At the beginning of this war we all thought the overwhelming weight of this Empire might quickly crush resistance out of what we considered to be two small countries; but we found that we were sadly misled. The unpreparedness under which we started this war, and which, as Sir Wilfrid Laurier said, redounds to the eternal glory of this country, brought us into unpleasant and unexpected difficulties. These, however, gave us electrical engineers a chance of showing what an intelligent organisation could do. Our corps was only an adjunct to an adjunct of the Army, yet they did the wonders of which Colonel Crompton has told us here to-night. When this war is finished, we shall have to start that bigger fight—not a fight against little countries, but a battle for the industrial supremacy of this country against an array of big nations. In this battle electrical engineers will not be an adjunct only: I think they will be looked upon as the leaders of the nation. It will be they who will be the general staff, who have to form the plans and the tactics for the nation, and they will have to find the ways and means of

communication, and will have to provision hundreds of millions of men. To do this the members of this Institution have a great duty to perform, and much work to do. We want to learn in this work from what we have heard to-night. I think men that have done what our Electrical Engineer Volunteers have done, who have risen to the emergency of the occasion in the manner we have heard, who have shown that they are willing to sacrifice everything to their patriotism, and have disproved that they have gone out to South Africa merely for the sake of a rough life, or for fighting, for a bullet or a medal, who have gone through all the hard drudgery of an engineer's work in a campaign, are fit to do any work. I think those men should be singled out, should be hall-marked, if I may put it in that way, for the rest of their lives for all the important positions that the work in which we are engaged may bring forth. I think it is the duty of every member of this Institution who is an employer to remember at all times the sterling qualities, the hard work, the perseverance, and the high sense of duty of this corps. It is our duty to help them to get those posts of honour and those remunerative positions, not only for what they have done, but also for the glory and the prestige that they have shed on this Institution. With these words I have much pleasure in seconding Mr. Siemens' proposal.

The PRESIDENT: Before putting the resolution to the meeting, I should like to say that that suggestion of Mr. Hirst's that the members of this Institution who are employers, and are in a position to do it, ought to remember that some members of the corps may be out of employment.

The resolution was put, and carried with acclamation.

Mr. J. W. SWAN: Colonel Crompton has made us realise that war has its bright as well as its sombre aspects; indeed, the lights of the picture he has presented to us so vividly to-night have almost overpowered the shadows of the picture. And, gentlemen, there are shadows that I think we must not overlook. We lately rejoiced in the return of a very large proportion of the men who went out to render their help in the defence of the honour of the nation, and in the protection of the lives and liberty of our kindred in South Africa. We rejoice that such a large proportion of those men who went out nine months ago have returned—returned safe and well, most of them

looking even better than they looked when they went away. But, gentlemen, not all the sixty-four men who went away have returned. All of those who went, no doubt, realised the perils that the occasion might place them in, and were prepared to meet the worst consequences, but only four had to make the sacrifice which they were all ready to make, to render up their lives in the service of their country. The names of these four men, honoured names—names, I trust, that will never be forgotten—are Second Corporal A. Holdaway, Sapper E. C. Short, Sapper W. C. Weakley, and Sapper E. J. West. "These few, these happy few," will never return. The manner of their death was pathetic rather than dramatic. It was not their fate to fall amidst the din and fury of battle, but their death was none the less heroic in that they died a lingering death in the hospital, stricken down at the post of duty not by Mauser bullets sped by the hand of an unseen foe, but by the deadlier stroke of that subtlest of enemies, that with sleuth-hound tenacity dogs the footsteps of armies in the field—enteric fever. Just as surely as the bullet sends the sorely wounded man to the hospital, so with equal sureness this lurking enemy strikes home, and exacts the same endurance of long-drawn-out suffering, and with even greater likelihood that all the skill of the doctor and all the care of the nurse may prove of no avail, and that the end of all is—a few freshly-turned sods on the veldt. But no, that is not all, for it is not true that death is the end of life: life and the deeds of life are ineffaceable. These few gallant members of a gallant band have the supreme honour of having died serving their country in a good cause. So dying, they have achieved immortality. They fell marching in the path of duty, and now, as ever, "The path of duty is the way to glory."

Let us always remember them, and let us also remember those to whom they were near and dear, and let us try to lift, if ever so slightly, the burden of their sorrow, by our sympathy. I beg to move, "That the Institution of Electrical Engineers desires to express its deep sorrow, and to tender its heartfelt sympathy to the relatives of Corporal A. Holdway and Sappers E. C. Short, W. C. Weakly, and E. J. West, who lost their lives while serving in the Corps of Electrical Engineers (Royal Engineers) Volunteers in South

Africa, and so gallantly devoting their technical skill to their country's need."

Sir HENRY MANCE: It has occurred to me that although there are many who would have seconded this resolution with greater eloquence, there is a certain fitness in asking me to perform the duty. I suppose I am the oldest Volunteer in the Institution of Electrical Engineers. I joined the movement in the fifties, and for many years before I left India I was an officer in the Volunteers there. There is another reason why I feel great sympathy with this evening's proceedings, namely, because I have two sons in South Africa, one of them an officer of the Royal Engineers, who was, I believe, on board the first train which entered Mafeking after the siege. I think that this resolution will be some consolation to the relatives of those who have lost their lives. I remember some time ago being at Southampton and witnessing the departure of some thousands of our troops, as happy as men could be. Among them were many men who were making great sacrifices, but when I walked away, with a rather heavy heart, I felt that those who were making the greatest sacrifices were the wives and mothers and the women of this country.

I beg to second the resolution.

The PRESIDENT: With Mr. Swan's permission, I should like to add one line to the resolution, namely, "and that the Secretary be instructed to communicate with their relatives."

The resolution, with the addition proposed by the President, was carried in silence, all the members present rising in their places.

The PRESIDENT announced that the scrutineers reported the following candidates to have been elected :—

Associate Members:

Eric Philip Barfield.

Charles Shore.

Student:

George Thomas Lundy.

MANCHESTER LOCAL SECTION.

Paper read at Meeting of Section, November 27th, 1900.

RELATIVE ADVANTAGES OF DIRECT-CURRENT AND THREE-PHASE DISTRIBUTION FOR SMALL INSTALLATIONS.

By HARDMAN A. EARLE, Member.

When accepting an invitation to read a paper before this Institution, I was for some time in doubt as to what subject I should choose; none was particularly uppermost in my mind as one upon which I wished to discourse, but I came to a decision while travelling abroad; the question covered by the title of my paper being brought most prominently before me, owing to the competition and different statements I met with, with regard to the relative advantages of three-phase and direct-current machinery for small and medium-sized installations. Owing to this, it occurred to me that a comparison between the two systems would be of interest and likely to give rise during its discussion to points which would be a guide for the future. Competition is not always, especially in some countries, carried on on strictly commercial lines, and I have experienced instances where a competing firm, finding it could not secure an order for direct-current machinery, has offered polyphase machinery as an alternative, singing its praises at great length, setting forth advantages without number, and naturally ignoring the other side of the question; this is not as it should be, but with inexperienced purchasers, with no one to guide them, it is at times successful.

I have found the title I first adopted for my paper somewhat too limited in its scope, and have briefly considered questions which relate to the distribution from substations, as well as that of the choice of machinery and distribution for small installations.

The type of machinery and the system to be adopted for supply and distribution are, in the case of large plants, most carefully considered; simplicity of design, first cost, and subsequent economy being among the most important of the many factors.

In the case of small installations, however, such as are put down to supply light and power for mills and factories, the question is not infrequently decided irrespective of theoretical considerations.

The purchaser in England is very usually in the hands of the consulting engineer, but in countries where this commodity does not exist he is often guided by the persuasive power of the seller, and sometimes by fashion. For the lowest price may mean but little as regards cheapness when there is no specification. There are many ways of preparing a low tender; and the omission of something in one which is included in another, the adoption of different voltages, various groupings of lamps and motors, different speeds for generators and motors, may give one system an apparent advantage over another which it does not in reality possess.

Many firms have, up to the present, given their chief attention to direct-current work, and this has met the wants of the smaller installations as well as those of the greater portion of the large central stations. Single-phase supply at high voltage, with distribution from transformer stations, has competed keenly with direct current, but lamps of higher voltage, together with three-wire distribution, has given the latter a long lease of life, and has enabled it to be employed for large areas without undue loss or a prohibitive cost of copper.

We have seen 50-volt and subsequently 100-volt lamps in regular use for large installations, but now 200 up to 230-volt lamps are quite the accepted standard, and this higher pressure has so largely reduced the first cost of copper that in many stations where the use of alternating currents was formerly warranted, direct-current machinery is being put down, and even in some instances the alternating-current machinery being taken out. Running and driven balancers, together with feeder boosters, are required as auxiliary machines for direct-current three-wire distribution, from substations dealing with large powers, and feeding over a considerable area; but it is more than probable that in the future 400-volt lamps will be available, which, with a two-wire distribution, would render the two first-named machines unnecessary, and eliminate the cost of the centre wire. A polyphase and monophase machinery lives on the price (

copper, it can be fairly assumed that every saving in this respect will take away their livelihood and reduce their utility. Where lighting constitutes the largest proportion of the load, the copper required is proportional to the watts per candle taken by the incandescent lamps, any reduction, therefore, in this respect is of great importance; lamps are, I understand, shortly to be put on the market consuming 2 watts per candle, or even less, and every advance in this direction is, I again hold, to the advantage of direct current, and will enable it to cover a wider field, for who will use high voltages and transform down, if they can use low with equal economy, or include complications when they may be omitted? All auxiliary machinery and apparatus has only one object, and that is to reduce the price of the mains, or, as I have seen it stated, adopted in the vain endeavour to cheat Ohm's law out of its due tribute of copper.

There are some who blindly champion alternating and polyphase systems, call commutators excrescences, and quote experiments which claim to show that polyphase weigh a mere fraction of direct-current machines. A statement in Herr von Dobrowolsky's remarks in the discussion on a paper read by Herr Görges in 1892 was quoted in the first edition of a book, and has again appeared in the second edition, namely, that a certain multipolar continuous-current machine gave 11,000 watts, but that when a three-phase armature was run in the same fields, it gave an output of 33,000 watts. I doubt this test being one which one could consider upon a commercial basis. On turning, however, to the paper in question, I find that Herr von Dobrowolsky also said: An alternating-current machine is so totally different in its construction to a direct-current machine, that such comparisons cannot be fairly made, and that it is equivalent to saying that a spoon has an efficiency of 100 per cent. when used for soup, but only one of 50 per cent. when used for cutting.

Notwithstanding all this, the polyphase system is at the present time the most satisfactory solution of long-distance transmission, and will be utilised more and more to transmit large powers into the centre of towns, and to cope with the many difficulties which present themselves; but the chief point I desire to raise is, whether the distribution from the substations should be by polyphase or direct current, and

whether for smaller installations one system offers such advantages over the other as to warrant a preference being given.

Notwithstanding the existence of several eminent advocates of single-phase machinery, I do not propose to consider it, nor have I had to deal with it in connection with the competition I have experienced, especially abroad.

With regard to the two remaining rival systems, the commutator seems to be the red flag of polyphase exponents, but any trouble that has been experienced on this behalf is merely a passing nightmare, and has only occurred with machines of large power running at high speeds, in connection with which considerable experience is required and very high class workmanship demanded in order to enable carbon brushes to maintain good contact on a surface running sometimes as high as 40 miles an hour; this difficulty has been experienced by most of us, but is being rapidly overcome, and in a short period it will be a thing of the past. It is, however, much to be desired that the high engine speeds which have been saddled upon us for plants of 1,000 horse and upwards may in the future be reduced some 20 per cent.

With reference to the comparative weights of direct-current and polyphase generators, we may take as a good example of the latter one of the 32-pole three-phase generators built for the Central London Railway, and which has an output on full load of 850 kw. at 94 revolutions; this machine has a weight of 37 tons, or, with exciter, say 39 tons. As an example of a large direct-current traction and lighting generator, we may take one of a number of 10-pole machines now being built at Salford Ironworks, having an output of 900 kw. with fixed lead, and running at 95 revolutions per minute. This machine has a weight of 45 tons, or, correcting for the increased output, it equals 42 tons; this machine could, however, have been built lighter as a shunt generator, with the compound windings omitted, and, with the smaller range of voltage then necessary, the magnets would have been lighter, and, probably with advantage, more in number; the weight would then have been practically identical with the polyphase generator, with which it has been compared.

With regard to the weights and prices of still lar

generators, I have compared a 1,400 kw. × cos ϕ polyphase generator running at 93 revolutions with a direct-current one of the same output, and, for the same speed, the former, with exciter, would weigh approximately some 64 tons, and the direct-current generator approximately the same. Comparing the prices of the above machines, the polyphase has the advantage and is approximately some 12 per cent. cheaper to build, this being chiefly due to the cost of the commutator on the direct-current machine.

There is, however, one point which cannot be passed by unnoticed, and that is the small safety clause attached to the output of the polyphase machine, namely, cosine ϕ—what is this ? Why it means that on an inductive load this apparent 12 per cent. advantage is wiped out, and may even, and very probably will, put the boot on the other foot; but more of this later on.

With regard to smaller sizes of 200 to 400 kw., I do not find that there is much difference; in fact, the figures I have tend to show that the advantage is on the side of direct current, but I will not press this point.

Respecting the weights of the two types of motors, the polyphase type with squirrel-cage rotor is considerably lighter, but the large motors of 50 to 100 horse-power with wound rotors weigh about the same.

Comparing the price at which the smaller sizes are sold, the polyphase have an advantage of about 5 per cent.; but figures I have carefully examined for motors from 10 to 100 horse-power with wound rotors show a good average of 10 per cent. in favour of continuous current, when compared with polyphase motors imported into this country, and the cost of importation does not amount to 10 per cent.

Considering next the cost or weight of mains, the advantage is on the side of three-wire direct-current distribution, as may be seen from the following table on pages 314 and 315.

In that table it is assumed that the various systems are for incandescent lighting as well as for power, and that the voltage is limited by the lamps, the circuits being so arranged that each lamp can be switched on singly.

As regards the voltage, column 1 gives the virtual volts between any two wires when lamps of the same pressure, namely 200 volts, are [illegible]n each system; but this

figure may certainly be taken exception to in the case of the alternating systems, for it must not be forgotten that their maximum voltage is the $\sqrt{2}$ times their working voltage, or 1·41 times as great. And this cannot be lost sight of when taking into consideration the questions of insulation and risk from shocks. I have, accordingly, given in column 2 the maximum voltage between any two mains on this basis.

There is also another point to be borne in mind in connection with the mains for polyphase systems which contain many motors, and that is for equal loss they must be increased in section at least 10 per cent. above that given in the table, and required for continuous-current systems, in order to allow for the loss due to wattless currents, and this figure may have to be still further increased to 20 per cent. or even more should many of the motors be put down to deal with very variable loads ; in fact, the section of the mains in the table must be increased in the inverse ratio of the power-factor of the system, taking into consideration both motors and lamps.

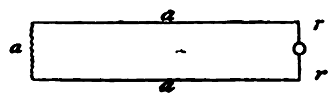
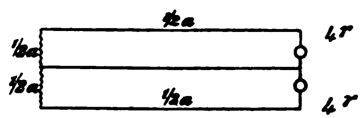
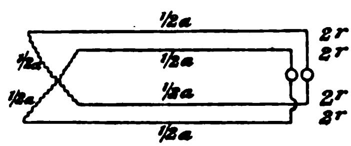
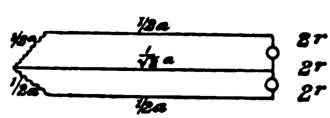
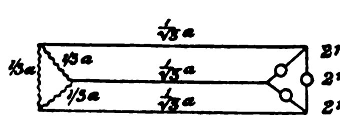
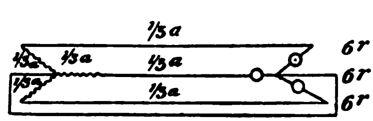
In the comparison, a third or common wire has been included, where necessary, to fulfil the condition of individual control, but the systems are assumed to be in balance, *i.e.*, there is no loss in the centre wire, which has been taken in all cases as 25 per cent. of the weight of an outer.

It is seen that continuous-current three-wire systems are only beaten by 1, namely, in the case of the three-phase star with common return, but then the saving by having only three wires instead of four to fix and insulate would cancel this small difference, not to mention the better regulation which can be obtained with continuous-current machinery.

With regard to this small difference of 1, the three-phase star has, moreover, no right to, for it is entirely eliminated, and the tables are turned, by what I have said respecting the loss due to wattless currents when motors are used ; and the three-wire direct-current system is then found to be at least 10 per cent. the best of all, as regards the weight of copper.

From the way I have drawn the diagrams of the three-phase star circuits, it can readily be seen why, when lamps are used, the fourth or common wire is necessary to maintain proper regulation of voltage on the lamps, for without

EQUAL PRESSURE ON THE LAMPS. EQUAL POWER

System.	Diagram of System. ~~~~ represents Dynamo windings. —o— " 200-Volt Lamps.	Virtual Volts between any two wires = V.	Maximum Voltage
Continuous Current 2 wires		200	200
Continuous Current 3 wires		400	400
2-phase 4 wires		200	$V\sqrt{2}$ 283
2-phase 3 wires		$200\sqrt{2}$ 283	$V\sqrt{2}$ 400
3-phase mesh		200	$V\sqrt{2}$ 283
3-phase star		$200\sqrt{3}$ 346	$V\sqrt{2}$ 490

TRANSMITTED. EQUAL LOSS IN THE LINE.

Current in outers for equal total power = 200a.	Resistance of each outer for equal total Loss = $2a^2r$.	Weight of each outer. — No current in centre wire.	Weight of the centre wire taken as $\frac{1}{2}$ of each outer.	Total copper for equal lamp pressure power and loss.	Weight of Copper cc. = 100.
a	r	1	—	2	100
$\frac{1}{2}a$	$4r$	$\frac{1}{4}$	$\frac{1}{16}$	$\frac{9}{16}$	28
$\frac{1}{2}a$	$2r$	$\frac{1}{2}$	—	2	100
$\frac{1}{2}a$	$2r$	$\frac{1}{2}$	$\frac{1}{2}$ same as outer	$1\frac{1}{2}$	75
$\frac{1}{\sqrt{3}}a$	$2r$	$\frac{1}{2}$	—	$1\frac{1}{2}$	75
$\frac{1}{3}a$	$6r$	$\frac{1}{6}$	$\frac{1}{12}$	$\frac{11}{24}$	27

the system would be comparable to a continuous-current two-wire system with three lamps in series.

Turning next to the question of running motors upon the circuits, the points to be considered are—

(1) Starting Torque. (3) Constancy of Speed.
(2) Regulation of speed. (4) Efficiency.

Polyphase motors are essentially constant-speed machines, and the speed of the smaller sizes is high, the arrangement of the winding is limited, and in this respect they do not so readily lend themselves, as continuous-current motors do, to windings by which any desired speed can be obtained. Various ingenious methods have been adopted to meet the requirements, which are more or less satisfactory, but there is still considerable room for further development.

1. STARTING TORQUE.

A continuous-current motor, even when shunt wound, exerts its full torque, with a small increase of current over the full load current, and is capable of satisfactorily dealing with any work for which it may be employed; small three-phase motors with squirrel-cage rotors, starting with full torque require a momentary rush of current, approximately equal to 2½ times the full-load current; this is an undesirable feature, and to obviate it motors of larger powers are started up with a resistance in the rotor circuit. This minimises but does not remove the defect.

This behaviour would prevent a three-phase power installation being arranged so that all motors may start up with the generator. With respect to this, I arranged a power installation of 850 E.H.P. in a large spinning and weaving mill abroad, in which are included some ten compound wound continuous-current motors, placed long distances apart, and ranging from 220 to 30 horse-power; in this installation all the motors can start up simultaneously with their generator, on practically full load, the starting current required being not more than some 50 per cent. in excess of the full working current.

In cases where the running torque is constant, but where a great starting torque is required, series wound continuous-current motors offer the best solution.

I have ‹ pecially in my mind, in connection

with a polyphase installation in a cotton mill, where a large number of motors of 8 B.H.P., with squirrel-cage rotors, were put down, each to run a line shaft driving 24 narrow looms; in this case trouble was experienced in starting up, belts coming off, or the motors, expecially on Monday morning, when everything was stiff, refusing to start at all. Larger motors were in some instances put in, and in others fast and loose pulleys were added: a 7-H.P. direct-current motor would have done the same work, and have given better results.

One method of increasing the torque of polyphase motors, on starting, is to increase the voltage on the stator by means of a booster transformer.

Another method is to alter the connections of the stator windings from star to mesh; thus increasing the pressure in the ratio of 1·73 : 1. Another arrangement is to change the connections of the stator coils from series to two sets in parallel.

The requisite starting torque can, however, be obtained from a direct-current motor, whether shunt or series wound, without any of these devices.

2. Speed Regulation.

The regulation of speed, either of continuous-current or of polyphase induction motors, is effected by somewhat similar means.

The speed of an ordinary shunt-motor can be varied in three ways, either by inserting a resistance in the armature circuit, by altering the voltage of supply, or by varying the strength of the fields.

Like shunt or series direct-current motors, polyphase motors, except the smallest sizes, require a resistance to be inserted in the rotor or revolving windings to keep the starting current within bounds. Merely by the use of such a resistance any desired variation of speed can be obtained with either type. With this method of regulation, however, the input remains practically constant, and it is therefore too wasteful to be adopted where a high average efficiency is imperative throughout the whole range of speeds.

A direct-current motor running on a three-wire system with two voltages available, combined with regulation of the field, can be made to yield a very high mean efficiency

equalling 83 per cent. for the whole of the working range—that is for some 66 per cent. of the speeds and an average of 75 per cent. throughout the whole range. Weakening the field by introducing resistance into the stator circuit of a polyphase motor increases the slip and the motor runs slower; but as the field supplies the rotor current in the same manner as the primary of a transformer supplies the secondary, the torque falls off very rapidly.

By doubling the number of poles in the stator winding the speed can be halved; this necessitates, however, an inconvenient number of windings, increases the complication of the switch, etc., and is rather impracticable.

Considering the various devices as a whole, one cannot but be drawn to the conclusion that the direct-current motor on a three-wire system, with two voltages available, or on a two-wire system with double wound armature, together with the provision for some field regulation, is the simplest and most desirable, for it gives all variations of speed most economically, and it can be easily arranged so that there is no jump whatever between one speed and another. Three or four instances have come to my notice where power has been transmitted to a works from a considerable distance by polyphase current, but in connection with which motor generators have been added to supply direct current to motors requiring to be operated through a large range of speed at a high mean efficiency.

3. Constancy of Speed under Varying Loads.

In this respect a shunt wound continuous-current motor acknowledges no superiority to the polyphase; the slip on the latter increases with the load, but shunt wound motors are available which can maintain constant speed from no load to full load.

For Tramway work a polyphase motor will maintain a more constant speed when climbing hills than a series wound continuous-current motor, but as this is at the expense of current, a high speed on the level and a slower speed on hills seems more to be desired, and likely to reduce the maximum current demanded from the station.

4. Efficiency.

I have compared the efficiencies of direct-current motors

which have been most accurately tested, with figures which I consider are fairly representative of what can be obtained with polyphase, and give the comparisons below :—

	3½	6	10	25	50	100 B.H.P.
Direct current	82% ...	85% ...	86% ...	91% ...	92% ...	92%
Polyphase ...	75% ...	80% ...	84% ...	88% ...	90% ...	91%

This shows a large superiority for direct-current motors of small power, and a small advantage on the larger sizes.

The power-factor of polyphase motors, as far as any published figures go, as well as those which are obtained from various makers, vary more widely than seems at all necessary, and in order to obtain a fair average, I dotted all the power-factors I could get upon a sheet of paper, and then drew a curve through the lot, and I give you below the readings which resulted from this curve.

Power-factor for polyphase motors on full load :—

B.H.P.	5	10	15	25	50	100
	·76 ...	·79 ...	·81 ...	·83 ...	·86 ...	·88

With regard to the above, it is needless to remark that the power-factor of continuous-current motors is unity.

Having considered how the various operations are effected for changing either the speed or torque or both, let us consider the conditions under which motors have to work; these may be divided under four heads and stated as follows :—

(1)	Constant	speed with	constant	torque.
(2)	„	„	variable	„
(3)	Variable	„	constant	„
(4)	„	„	variable	„

Constant Speed with Constant Torque.

This condition is one which exists, when motors are used to drive lengths of shafting, from which again are driven a considerable number of small machines, such as are found in a cotton mill, or a large machine shop; in this instance the load is fairly constant, possibly not varying more than some 10 per cent.; it is in such cases that a polyphase motor is at its best, and endeavours to rival the shunt wound direct-

almost [illegible] which, however, is just as good for the purpose.

Constant Speed with Variable Torque.

This condition will occur in cases where large portions of the load are being frequently thrown on and off; the shunt wound motor is all that can be desired as regards speed on such work, and the current taken is in fair proportion to the load. The polyphase motor is also satisfactory as regards speed, but the current is high on the lighter loads; a considerable percentage is certainly wattless current, but it adds to the C^2R losses, and at the same time decreases the capacity of the generator for useful work, and this is a point which must be borne in mind when comparing a polyphase generator with a direct-current one.

We have seen how a power-factor less than unity for the motors necessitates an increase in the size of the mains, and this acts in a similar manner upon the generator. If the current were in phase with the E.M.F.: that is, if the power-factor were 1, in place of averaging, say, ·85, the polyphase generator could do $17\frac{1}{2}$ per cent. more work than it would be able to do under the latter condition. But, again, with many motors working on a system, a power-factor of ·85 cannot always be relied upon, and one of ·75 or even lower may have to be reckoned with. We see from this that the first cost of an installation may be considerably increased by this consideration, and, besides this, the wattless currents exercise a very bad effect on the regulation of a three-phase machine, very much more so than if the currents were in phase, necessitating very much heavier windings on the magnets to deal with a low power-factor than a high one.

Variable Speed with Constant Torque.

This third condition is met with very frequently, for instance, when driving callenders, paper-making machinery, and in pumping, etc. To meet it with polyphase motors the regulating switches are more complicated, and to effect many changes of speed the groupings of the windings become many in number and efficiency is sacrificed; in this respect the continuous-current motor is more easily managed [illegible] a higher mean efficiency.

VARIABLE SPEED WITH VARIABLE TORQUE.

The fourth condition is especially present when driving calico printing machinery; an eight-colour machine, for instance, may be utilised for printing any desired number of the eight colours; the torque increasing practically in direct proportion to the number of rollers in use, the speed of printing the goods at the same time varies in accordance with the number of colours being printed, and with the class of work the machine is doing.

For this class of work a very even turning moment is necessary; one must be able to run up to any desired speed without jumps or jerks, and the contemplation of a three-phase motor arranged for high efficiency at all speeds and to comply with the requisite conditions, is not a taking one.

I have heard the point raised that for factories where there is much inflammable dust about, a chance spark from the commutator might set the place on fire; but perfectly satisfactory entirely closed direct-current motors are at work, not considering the thousands on tramcars, and in factories of all descriptions. Fire-proof gauze covers are also employed for motors; these do not exclude all ventilation, and render the motor quite reliable even where spirit vapours are present.

The facility for the use of batteries on direct-current systems is one which must receive careful consideration. These are continually improving in quality, give excellent results in the hands of those that understand them, and offer great advantages, both as a stand-by and as regulators; they can therefore frequently be utilised to assist direct-current supply, and at times to effect considerable economy, whereas a three-phase battery has yet to be invented.

Polyphase motors appear specially suitable for long lines of railway where considerable distances are travelled without a stop, at a uniform rate of speed, and where crossing points, which would complicate the conductors, do not exist, for beyond a comparatively limited distance high voltage must be used, and then, whichever system is employed, transforming apparatus is necessary; in such instances polyphase supply, with polyphase motors on the train, would not call for rotating transformers, stationary transformers along the line being the simplest and cheapest arrangement,

Such a system is, however, unsuitable for use in towns, with frequent stops and a very variable rate of speed; besides this, I am sure that it would create a great commotion among the inhabitants of a town not 100 miles away, if it were proposed to erect two trolley wires in place of the one, which to some already appears distasteful. Judging from what some of the larger firms on the Continent are doing with respect to polyphase machinery, I understand that although they have given it the very greatest attention, they find that the volume of their direct-current work exceeds that of the polyphase.

I have endeavoured to consider the chief points which should guide us in the choice of a system for use for the smaller installations.

In cases where lighting alone is required, the first cost of polyphase or direct-current would appear to be very similar. The simplicity of the latter system, both as regards the wiring and the regulation of the voltage, to my mind, gives direct current the preference.

Where a number of motors are on the circuit, for whatever purpose they may be required, and whether the system includes lighting or not, I consider that direct current shows a very decided advantage except in a few special cases; moreover, the use of batteries is all but excluded with polyphase systems, even for a night load, and this is frequently of considerable importance. I have, doubtless, to some minds left unsaid many things I ought to have said, and to others said many things I ought not to have said, but this may be more an advantage than otherwise, for it will admit of those shortcomings being criticised during the discussion, and may enable me to fill the gaps and correct my errors.

Mr Miller. Mr. THOS. L. MILLER agreed with Mr. Earle in his conclusions as to the advantages direct-current possesses over polyphase transmission for small installations, and particularly where speed regulation of the machines was a matter of importance. Polyphase transmission had undoubtedly come very much to the front of recent years; but while the system had very considerable advantages where energy was transmitted over considerable distances, where the flexibility of the system could be utilised to the utmost, he thought that for small installations direct-current tr[illegible]ad, in the majority of cases, decided advantages over i[illegible] had pointed out, in this matter—as in so many othe[illegible]nstanced—fashion, and that idea so

prevalent in some quarters that nothing good—electrically—could be produced in this country, had had much to do with the spread of polyphase transmission over comparatively short distances in these parts. Much was being said at the present time about the spread of polyphase transmission abroad; but the speaker thought that in many cases the conditions under which this work was being, or had been, carried out had been entirely lost sight of. A study of the published records of the work carried out on this system, both on the Continent and in America, showed that in the majority of instances either cheap power or transmission over comparatively long distances, or both combined, had been the chief factors in determining its adoption. Mr. Miller.

Mr. Earle's table giving the weight of copper used in the various systems was very interesting; but in the majority of cases of electrical transmission in small installations the speaker did not think that the weight of copper used in conductors had much influence in determining the system to be adopted. Where no great speed variation was required he was of opinion that, for small installations, a simple two-wire direct-current system had much to recommend it, one of the chief advantages being that it could be put in charge of any ordinary mechanic.

With regard to the larger question of electrical distribution in cities, touched upon by Mr. Earle, he (Mr. Miller) did not think any general statement of opinion could be given, as so much depended upon the special circumstances of each particular case—and until these were known it was better to suspend judgment; but for small installations for factories, mills, and works, he cordially agreed with Mr. Earle in his conclusions as to the advantages of direct-current transmission.

Mr. W. B. SAYERS spoke of the great value of a delicate speed adjustment, with practically no loss of efficiency, such as was possible with shunt-regulated direct-current motors when applied to machine tool driving. He pointed out that production could be materially increased by a careful use of this property of the direct-current motor. He instanced a case in which Messrs. Ganz & Co. had been obliged to instal two generators with different periodicities, and, by means of a change-over switch, arrange for any motor to have either periodicity applied to it, with a consequent variation of speed. Mr. Sayers.

Mr. A. P. WOOD referred to the drop of pressure which he had observed take place in the supply circuit when a 10-H.P. polyphase motor was switched on to a 100-kw. generator. He had measured it—110 volts down to 70 volts. He then referred to the microscopic clearance in polyphase motors, and said that sooner or later failure must result therefrom—especially in dusty places. Mr. Wood.

Mr. LINDLEY said that the multiphase motor appeared to him to be an ideally perfect machine from a mechanical point of view. It was all very well to say that commutators did not give trouble. He was a user of direct-current motors, and the trouble was very considerable. He had many makes in his works. Mr. Lindley.

Mr. A. WHALLEY regretted that judgment against polyphase motor appeared likely to go by default at this meeting because none of t

Mr. Whalley. few experts in this country were able to be present. So far the discussion had ignored the chief assumption of the paper, that power would be generated and transformed at substations by some polyphase apparatus, which meant that if direct-current motors were adopted, rotary converters were needed in the substations and, in addition, a low-tension network. The conditions which would warrant the adoption of polyphase motors in order to avoid the expense of a low-tension network and the use of rotary converters needed further debate.

It appeared to him too late to discuss merely the technical merits of the rival systems. The matter was now a commercial one, as there was abundant business to be done in the world in polyphase apparatus, and up to the present this has been the only manufacturing country not competing for such business, whether due to a lack of the necessary commercial or technical experience, or of sufficient skilled workmen, or of the necessary manufacturing capital under our free trade conditions. The foreign firms, whose activity has led to this paper being read, supplied both types of motor, and whatever might be the correct answer to the questions raised by the author, it would not reduce their competition.

The stronghold of the direct-current motor appeared to be its efficiency; but this term is unfortunate in its abuse, there being many definitions, nearly all of which ignore light-load losses, and none are complete enough to indicate the nature of the compromises which have been made in the design of a motor. Prompt delivery, proper testing before delivery, real interchangeability of parts, and the durability of the motor-starting gear, in addition to the life and the efficiency of the motors themselves, interested purchasers, and it was not easy to get satisfaction on all these points. There appeared to be too many makers of direct-current motors in this country, *i.e.*, too many relatively small workshops manufacturing motors, judging by the success of the few—yet large—foreign firms.

Mr. Wyld. Mr. W. Wyld, as one in charge of a three-phase plant, desired to say a few words. One speaker had told a terrible tale of how he saw the voltage vary on a polyphase plant on the Continent from 150 to 70 volts, but he had experienced nothing of that sort on his plant. He had some 500 H.P. of motors and about 400 incandescent lamps and 60 arc lamps, and by having this lighting pretty well balanced he did not experience any great variation in the voltage. His recording voltmeter sheets would, he thought, compare very favourably with those of any public supply central station. He ran his three-phase generators in parallel, and had not experienced the slightest trouble in this respect; they ran perfectly in parallel. It was comforting to hear Mr. Lindley speak of the polyphase motor as being mechanically ideal. One speaker had mentioned the small air space in the polyphase motor as liable to get filled with dust, etc. He had had no trouble from this, and there was not much chance of dust lodging in the space, as the draught caused by the rotor will not allow it to lodge there. With regard to the variation of speed, there were several cranes of from five to twenty tons worked by three-phase motors, and he had no trouble with the

speed regulation. This regulation was only used for short periods, so that the inefficiency of which so much has been said was not nearly so serious as was supposed. With regard to the paragraph in which Mr. Earle said that most of the Continental manufacturing firms are now engaged more on continuous than polyphase plant, he thought there was a general consensus of opinion among the members of the Institution who visited Switzerland last year that the Continental firms were more engaged upon polyphase plant, and in Mr. Fedden's Sheffield report, which had already been mentioned, it was stated that the German firms were more engaged on polyphase, particularly two-phase, than on continuous-current plant. In conclusion, he wished to express his certainty that had they adopted continuous instead of three-phase current the plant could not have given better results, even if it had given as good as had been obtained. Mr. Wyld.

Mr. H. EARLE, in reply, regretted that those interested in three-phase systems had not been more strongly represented in the discussion. He was much gratified that Mr. Miller, who, he knew, had carefully compared the two systems, agreed with him in the general conclusions he had drawn. Mr. Sayers' remarks as to the value of shunt regulation were very important, and he had himself adopted this economical method to increase the speed as much as 25 per cent. without unduly increasing the cost of the motor, and it admitted of very fine regulation without large resistance. Mr. Earle.

With regard to Mr. Wood's remarks respecting the small clearance in polyphase motors, this varied considerably, and it appeared to him that the difference allowed in the air-space by manufacturers was largely the cause of the varying power-factors met with.

He could not agreed with Mr. Lindley that continuous-current motors gave trouble, for if they were of good design and received proper attention they were to be entirely relied upon.

Mr. Whalley's contention that all unnecessary complication should be avoided was a right one ; first costs had, however, to be considered, and where high-tension transmission and low-tension supply were adopted three-phase and direct current would have to be combined, till three-phase motors of a high power-factor were produced and they were so improved that they could compete on an equal footing with direct current.

Firms in this country had naturally turned their attention to that for which there was the best market, the necessary technical experience was available to take up any system which might prove to be the best, but up to the present time direct-current distribution had held its own against its rival.

He could not agree with Mr. Whalley respecting the unreliability of tests and figures of efficiency ; these matters were most carefully attended to by all good firms, and it was only by attention to such matters that improvements in design could be effected, and makers were willing and able to supply motors to the strictest specifications.

Mr. Wyld's remarks were interesting, and he quite agreed that three-phase installations gave every success, though his examination of the subject had convinced him that when all points were considered direct-current offered many advantages.

ORIGINAL COMMUNICATION.

THE REGULATION OF THE POTENTIALS TO EARTH OF DIRECT-CURRENT MAINS.

By ALEXANDER RUSSELL, M.A., Member.

Introduction.—The regulation of the potentials to earth of the mains of a three-wire system of supply has become a subject of pressing importance, owing to the general adoption of higher pressures of supply. So long as the pressure between the outers of a three-wire system of supply was not greater than 250 volts, it was of no great importance what the pressure to earth of any of the mains was, as it was necessarily less than the Board of Trade limit of 250 volts. In this case it is inadvisable from the station engineer's point of view to alter the potentials of the mains to earth as, owing to electrical laws, they always assume the potentials that make the energy wasted in leakage currents a minimum. When, however, the difference of pressure between the outers is greater than 250, it is necessary to take reasonable precautions so that the potential from earth of any main may never rise for any lengthened period above 250 volts.

Mr. Wordingham has recently discussed the regulation of the potentials of the mains in his paper, "On the Maintenance of Certain Portions of Distributing Systems at Earth Potential," read at the Cardiff Convention of the Municipal Electrical Association, in the Summer of 1900. He there gives a clear description of certain causes which tend to lower the insulation resistance of the negative outer of a three-wire system.

For this reason the potential of the positive outer from earth is generally much higher than the difference of pressure between adjacent mains. In a large three-wire system, for example, the potentials of the mains are 190, 85, and −20 volts from earth respectively. If the pressure of supply were doubled, it is obvious that some method of lowering the potential of the positive outer would have to be adopted.

It will be instructive, theref[illegible]nsider how these

potentials depend on the fault resistances of the mains, and what methods should be adopted for regulating them. It is a prevalent opinion that the measurement of the insulation resistance of a distributing network is a difficult process. It is in reality a very simple process, and many easy methods of doing it will be found described in Mr. Raphael's book on "The Localisation of Faults in Electric Light Mains." Another erroneous idea is that a knowledge of the insulation resistance of a network is of little importance. This is not the case, as it gives us definite information. It tells us, for example, that the power being wasted in leakage currents is less than a certain amount, and it also places a superior limit to the values of the leakage currents. As the Board of Trade rightly insists that the leakage current must not exceed a certain maximum value, any short way of finding a maximum possible value to the actual leakage currents is of importance, for if this maximum possible be less than the Board of Trade limit, the network obviously satisfies the regulations.

Fault Resistance.—By the fault resistance of a main we mean the resistance of all those stray paths from it to earth which do not pass through the other mains. Suppose the shunt coil of a badly insulated meter to be connected between the positive and the middle main, and suppose the potential of the latter to be positive, then the resistance of the leakage paths from this shunt coil to earth will all be credited to the fault resistance of the positive outer. If, however, it were connected between the negative outer and the middle, then some of the leakage paths of the shunt coil would be credited to one main, and the remainder to the other. We shall see later that an alteration in the fault resistance of the positive main will alter the potentials of them all, and theoretically will alter the fault resistances of the other two, owing to the point of zero potential on the wires connecting the middle and negative mains shifting. Again, theoretically speaking, every time a switch is turned on or off the fault resistances will be altered. When we say, then, that the fault resistances of the three mains are f_1, f_2, and f_3 respectively, it must be borne in mind that they may vary at different times of the day.

Insulation Resistance.—If f_1, f_2, and f_3 be the resistances of the three mains, and F be the insu

resistance of the network, then F is defined by the equation—

$$\frac{1}{F} = \frac{1}{f_1} + \frac{1}{f_2} + \frac{1}{f_3}.$$

We should expect F to remain approximately constant in practice, for when a switch is turned on between the positive and the middle, some of the leakage paths are taken from one main and given to the other. f_1 may be diminished and f_2 increased, but $\frac{1}{f_1} + \frac{1}{f_2}$ would remain approximately constant. Where, however,

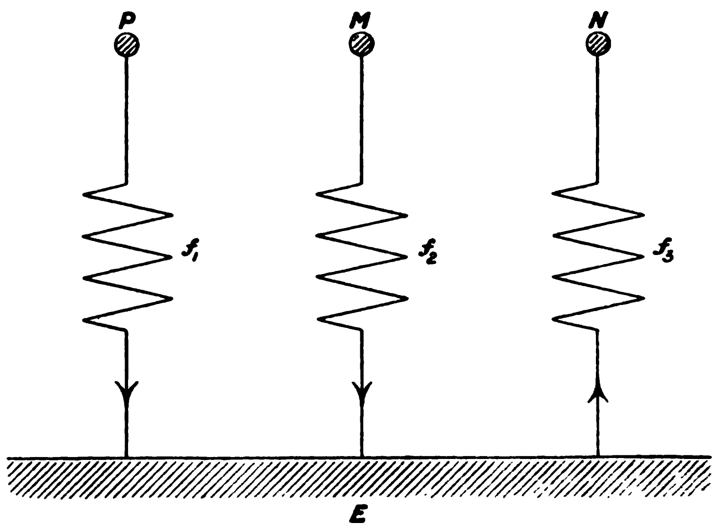


FIG. 1.

a double-pole switch is used for a leaky circuit, then, of course, F is diminished when the switch is turned on.

The Potentials of the Mains.—Let f_1, f_2, and f_3 be the fault resistances of the three mains P, M, and N (Fig. 1). Let also V_1, V_2, and V_3 be their potentials.

Then, by Kirchhoff's law—

$$\frac{V_1}{f_1} + \frac{V_2}{f_2} = \frac{V_3}{f_3} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

If V be the P.D. between P and M and also between M

and N, then, since the dynamos maintain these P.D.'s constant—

$$\left.\begin{aligned} V_1 &= V_2 + V \\ V_3 &= V - V_2 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Substituting for V_1 and V_3 from (2) in (1), we easily find V_2 in terms of V, f_1, f_2, and f_3, and hence from (2) we also find V_1 and V_3.

Graphical Construction for the Potentials of the Mains.— Draw a line P N (Fig. 2) and make P M = M N = V.

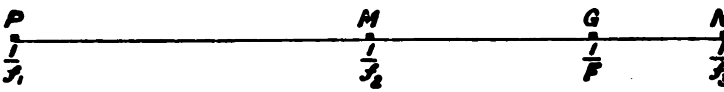


FIG. 2.

Place particles of mass $\frac{1}{f_1}$, $\frac{1}{f_2}$, and $\frac{1}{f_3}$ at P, M, and N respectively. Find their centre of gravity G. Taking moments about G, we see that—

$$\frac{PG}{f_1} + \frac{MG}{f_2} = \frac{NG}{f_3}.$$

$$\left.\begin{aligned} \text{Also} \quad PG &= MG + V \\ \text{And} \quad NG &= V - MG \end{aligned}\right\}$$

Comparing these equations with (1) and (2) above, we see that $PG = V_1$, $MG = V_2$, and $NG = V_3$.

In general, if we have *n* mains whose fault resistances are $f_1, f_2, f_3, \ldots$ and whose P.D.'s are maintained at $V, V', V'', \ldots$ respectively, then the potentials of the mains are given by the following construction. Draw a straight line and mark off along it points $P_1, P_2, P_3, \ldots$ whose distances apart are $V, V', V'', \ldots$ Place particles of mass $\frac{1}{f_1}, \frac{1}{f_2}, \frac{1}{f_3}, \ldots$ at $P_1, P_2, P_3, \ldots$ respectively, and let G be their centre of gravity, then—

$$V_1 = P_1 G, \; V_2 = P_2 G, \; \ldots$$

Measurement of Insulation Resistance.—Read the voltage to earth of the middle main by means of an electrostatic voltmeter. Connect the middle main to earth by

ammeter in series with a resistance. Let V_2 be the initial reading of the voltmeter and V'_2 the reading when the middle wire is earthed. Let also C be the reading on the ammeter, then—

$$F = \frac{V_2 - V'_2}{C}.$$

Example.—Initially V_2 was 112. When the middle was earthed, C was equal to 3·5 amperes and V'_2 was 8 volts, hence—

$$F = \frac{112 - 8}{3{\cdot}5},$$
$$= 29{\cdot}7 \text{ ohms}.$$

Proof of Formula.—Let PG, MG, and NG (Fig. 3) represent the initial potentials of the mains. We have

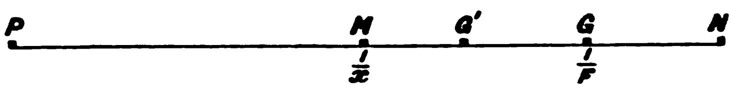


FIG. 3.

seen that G is the centre of gravity of masses $\frac{1}{f_1}$, $\frac{1}{f_2}$, and $\frac{1}{f_3}$ placed at P, M, and N respectively. We can replace these masses by a mass $\frac{1}{F}$ placed at G. Suppose now that a resistance x is connected between the middle and earth, then to find the new potentials we have to find the centre of gravity G′ of masses $\frac{1}{x}$ at M and $\frac{1}{F}$ at G.

Taking moments about G′—

$$\frac{MG'}{x} = \frac{GG'}{F};$$

$$\therefore \frac{V'_2}{x} = \frac{V_2 - V'_2}{F}.$$

But $\frac{V'_2}{x}$ is the current through the ammeter (C), hence—

$$F = \frac{V_2 - V'_2}{}.$$

Another Method.—When V_2 is small, it is often more convenient to earth the positive outer through a resistance x. Let C be the current in this resistance, then we can prove in exactly the same way as we did the preceding formula that—

$$F = \frac{V_1 - V'_1}{C}.$$

Keeping Down the Potential of the Positive Outer by Earthing it or the Middle Main through a Resistance.—Suppose that we connect the positive outer to earth through a resistance x. To find the new potentials, we have to find the centre of gravity of masses $\frac{1}{x}$ and $\frac{1}{F}$ placed at P and G respectively (Fig. 4).

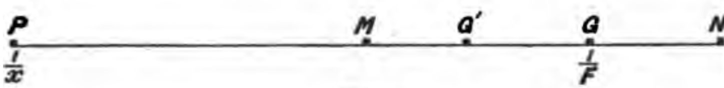


FIG. 4.

Taking moments about G′, we have—

$$\frac{V_2 - V'_2}{F} = \frac{V + V'_2}{x}.$$

But $\frac{V + V'_2}{x}$ is obviously the current through x, hence—

$$V_2 - V'_2 = F \cdot C,$$

where C is the current in the artificial leak.

Again, to find the resistance x to be put in between the positive outer and earth in order to lower the potential of each main by v volts, we have—

$$C = \frac{v}{F} = \frac{V + V_2 - v}{x};$$

$$\therefore x = \frac{V + V_2 - v}{v} F.$$

If there was a dead earth on the negative outer, the maximum current through the resistance x would be $\frac{2V}{x}$.

Similarly we can show that if we earth the middle

wire through a resistance x, and if the current flowing through it be C, then—

$$C = \frac{V_2 - V'_2}{F}$$
$$= \frac{V'_2}{x}$$
$$= \frac{V_2}{F + x}.$$

Hence, as before—

$$V_2 - V'_2 = F.C.$$

To lower the pressure, therefore, by v volts, the resistance of the earth connection of the middle wire must be $\frac{V_2 - v}{v} F$ ohms. The current that will flow in it will be $\frac{v}{F}$ amperes. If there is a dead short on either of the outers, the greatest current through the earth connection will be $\frac{V}{x}$ amperes.

Example.—Suppose that initially $V_1 = 300$, $V_2 = 100$, and $V_3 = -100$ volts, and suppose also that $F = 10$ ohms. It is required to calculate the resistance x of the earth connection to be put (1) between the positive outer and earth, and (2) between the middle main and earth, in order to reduce the pressure of the positive outer to 250 volts.

(1) By the above formulæ—

$$x = \frac{100 + 200 - 50}{50} . 10$$
$$= 50 \text{ ohms.}$$

In this case, if the negative outer were dead-earthed, the greatest current through x would be 8 amperes.

(2) When x is put between the middle and earth, then to lower the potential 50 volts we must have—

$$x = \frac{100 - 50}{50} . 10$$
$$= 10 \text{ ohms.}$$

If either of the outers be short-circuited, the maximum possible current through x is 20 amperes.

The expenditure of power in the first case would be greater than in the second by VC watts, *i.e.*, by a kilowatt. This, however, might be utilised at the station.

The Energy Expended in Earth Currents in the Mains of Distributing Systems.—The algebraical expression for this energy is—

$$\frac{V_1^2}{f_1} + \frac{V_2^2}{f_2} + \frac{V_3^2}{f_3}.$$

If we use the graphical construction shown in Fig. 2, it is—

$$\frac{PG^2}{f_1} + \frac{MG^2}{f_2} + \frac{NG^2}{f_3}.$$

Now if we regard the position of G as variable, then by a well-known statical theorem this expression is a minimum when G is the centre of gravity of masses $\frac{1}{f_1}$, $\frac{1}{f_2}$, and $\frac{1}{f_3}$ placed at P, M, and N respectively. But we know that this is how the potentials adjust themselves in practice, and hence they adjust themselves so that the energy expended is a minimum. This theorem is true for n-wire systems. Let x be the potential of the positive outer, then the energy expended in leakage currents is—

$$\frac{x^2}{f_1} + \frac{(x - V)^2}{f_2} + \frac{(x - V - V')^2}{f_3} + \ldots.$$

Now by Kirchhoff's law—

$$\frac{x}{f_1} + \frac{x - V}{f_2} + \frac{x - V - V'}{f_3} + \ldots = 0.$$

But by the Differential Calculus, this is the condition that—

$$\frac{x^2}{f_1} + \frac{(x - V)^2}{f_2} + \frac{(x - V - V')^2}{f_3} + \ldots.$$

should be a minimum, hence the theorem follows.

Again—

$$\frac{V_1^2}{f_1} + \frac{V_2^2}{f_2} + \frac{V_3^2}{f_3} = \frac{(V + V_2)^2}{f_1} + \frac{V_2^2}{f_2} + \frac{(V_2 - V)^2}{f_3}$$

$$= \frac{V^2}{f_1} + \frac{V^2}{f_3} + \frac{V_2^2}{F}.$$

Now if we connect the middle wire directly to earth, the power expended in earth-leakage currents is $\frac{V^2}{f_1} + \frac{V^2}{f_3}$. The loss of power in earth currents has therefore been increased by $\frac{V_2^2}{F}$. In the example given above, V_2 is 100 and F is 10, hence the extra power lost by earthing the middle wire is a kilowatt. In some of the older 100-volt three-wire systems, F is only two or three ohms, and there have been cases where it has even been less. In these cases the loss of power entailed by earthing the middle wire would be appreciable.

If we earth the middle wire through a resistance x, then the new values of V_2 and F are given by the equations—

$$V'_2 = V_2 \frac{x}{F + x}$$

and—

$$\frac{1}{F'} = \frac{1}{F} + \frac{1}{x}.$$

Therefore the increased loss of energy in this case is—

$$\frac{V_2^2}{F} - V_2^2 \frac{x^2}{(F + x)^2}\left(\frac{1}{F} + \frac{1}{x}\right) = \frac{V_2^2}{F + x}.$$

Effect of Earthing the Middle Main through a Battery.—Suppose that the middle main is at positive potential. Connect the negative pole of a battery to it (Fig. 5) and the positive pole through a resistance to earth. Let E be the E.M.F of the battery, and let x be the resistance from the middle main to earth through the battery. Then by Kirchhoff—

$$\frac{V'_2 + E}{x} = \frac{V'_3}{f_3}.$$

Exactly as before we can show that if we draw a line P M N and make P M = M N = V (Fig. 6), and also M X = E, then, if we place masses $\frac{1}{f_1}$, $\frac{1}{x}$, $\frac{1}{f_2}$, and $\frac{1}{f_3}$ at

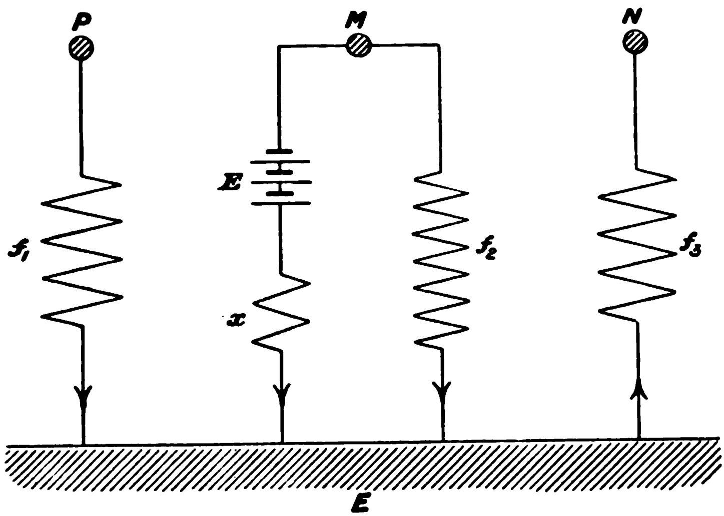


FIG. 5.

P, X, M, and N respectively, and if we find G′ the centre of gravity of these masses, then—

$$V_1 = PG'; \quad V_2 = MG'; \quad \text{and } V_3 = NG'.$$

Let G be the centre of gravity of $\frac{1}{f_1}$, $\frac{1}{f_2}$, and $\frac{1}{f_3}$, so that

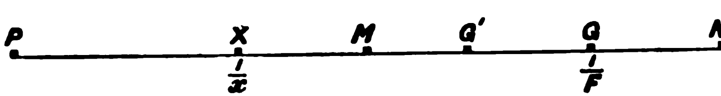


FIG. 6.

we may suppose $\frac{1}{F}$ placed at G, then—

$$PG = V_1; \quad MG = V_2; \quad \text{and } NG = V_3.$$

Taking moments about G′—

$$\frac{V_2 - V'_2}{F} = \frac{V'_2 + E}{x}.$$

Hence—

$$V'_2 = \frac{\frac{V_2}{F} - \frac{E}{x}}{\frac{1}{F} + \frac{1}{x}} \quad . \quad . \quad . \quad . \quad . \quad (a)$$

Also the current C through the battery is given by—

$$C = \frac{V'_2 + E}{x} = \frac{V_2 - V'_2}{F} = \frac{V_2 + E}{F + x} \quad . \quad . \quad (b)$$

We have supposed that E is acting in the same direction as V_2. If we connect the battery so that E opposes V_2, then we shall have to alter the sign of E in formulæ (*a*) and (*b*).

Formula (*a*) shows us that by means of a battery we can lower the potential of the mains more rapidly than we could by means of the resistance x alone. If we choose E so that it is equal to kV_2, and x so that it equals kF, where k is any number, then V'_2 will be zero. For example, if V_2 equals 50 and F equals 100, then making E equal to 25 and x equal to 50, we could reduce the middle wire to zero potential. In this case the current through the battery would be half an ampere, and the power it would be giving out would be 12·5 watts. In general, when the middle wire is at zero potential, the current through the earth connection will be $\frac{V_2}{F}$ amperes, and if there is a battery in this circuit, the power it will be expending will equal $E\frac{V_2}{F}$.

In several cases in practice it appears to be desirable to keep the middle main negative to earth. This can be done by a battery as above, or by connecting the positive outer to earth through a resistance. The expenditure of power in the earth connection in the latter case is to its expenditure in the former case as $V - e$ is to $E - e$, where $-e$ is the potential of the middle main. Another way of keeping the potential of the middle wire negative would be to use a motor-dynamo, the motor terminals being connected across the two outers and the dynamo terminals ' ries with the earth connection of the middl

Partial Earthing of the Outers.—One of the most effectual methods of regulating the potentials of the mains would be to connect the middle to earth through a very small resistance, and to have two variable resistances connected between the outers and earth. In the event of an earth occurring in either outer, then by cutting down the resistance between the other outer and earth we could regulate the potentials. In the case of a bad earth, considerable power would have to be expended in earth currents, and as these currents might cause serious damage, either by starting a fire or electrolytically, we shall consider the values of the leakage currents that are at present considered allowable.

Leakage Currents.—It is important to notice that all the methods described of preventing the potentials of the mains from rising above 250 volts increase the leakage currents to earth. As these currents are always flowing, it is desirable to keep them as small as possible owing to the electrolytic damage they may do. In the Board of Trade conditions for the approval of earthing it is provided that a record of the current to earth through the earth connection shall be kept, and that if at any time it exceeds the one-thousandth part of the maximum current of supply, immediate steps shall be taken to improve the insulation of the system. Now the current that is measured in this case is the difference between the leakage currents from the positive and negative mains. If the fault resistances of the whole three mains be very bad, as has sometimes happened owing to the insulating rubber having become rotten, yet if the fault resistances of the two outers be nearly equal, then the current in the earth connection may be very small, or it may even be zero. A better rule would be to insist that the insulation resistance "F" of the network, when the earth connection is removed, must always be above a certain amount. In order to get an idea of what this value of "F" should be, we will discuss the eighth of the Board of Trade Regulations (A). This regulation is as follows :—

> "8. *Maintenance of Insulation.*—The insulation of every complete circuit used for the supply of energy, including all machinery, apparatus and devices forming part of, or in connection with, such circuit, shall be so maintained that the leakage current shall not under any

conditions exceed one-thousandth part of the maximum supply current; and suitable means shall be provided for the immediate indication and localisation of leakage. Every leakage shall be remedied without delay.

"Every such circuit shall be tested for insulation at least once in every week, and the undertakers shall duly record the results of the testings.

"Provided that where the Board of Trade have approved of any part of any electric circuit being connected with earth, the provisions of this regulation shall not apply to that circuit so long as the connection with earth exists."

Now for a two-wire system this rule is clear. It proceeds on the assumption that the permissible leakage power must always be the same fraction of the total output. In other words, if we double the pressure of supply (the output remaining the same) the lowest permissible value of the insulation resistance is increased four times. Hence systems of supply are treated equitably from the fire-risk point of view. The systems, however, that use lower pressures are allowed to have larger leakage currents, and in the course of time the electrolytic damage done by them may be appreciable. It is desirable also that even when systems are dead-earthed through a connection, that this connection should be removable, so that the insulation could be tested once a week.

When we come to apply the above rule to three-wire networks we are met with the difficulty that the determination of f_1, f_2 and f_3 is not easy, and hence there is no simple rule to give us the earth-leakage currents from the three mains. There is also no practical method of determining the leakage currents that do not pass through earth. What we can determine easily is F the insulation resistance to earth of the network, and the potentials V_1, V_2 and V_3 of the three mains. The question then arises, having given the potentials of the mains and the insulation resistance of the network, can we determine the maximum possible value of any one of the earth-leakage currents, and the maximum possible value of the leakage watts expended in producing them? If we know these maximum values, then we can fix on a minimum permissible value to "F" in any given case. Of course it would be more satisfactory actually to deter-

mine the values of f_1, f_2 and f_3, but the ordinary methods are tedious and not very accurate in practice.

Let V be the P.D. between adjacent mains, V_2 the potential of the middle main, and "F" the insulation resistance of the network. We have to find out what the maximum possible earth current is in this case. It is easiest to do this graphically. Looking at Fig. 2 we see that G is fixed, and so also is the sum $\left(\frac{1}{F}\right)$ of the three masses. We want to find when $\frac{V_3}{f_3}$ is a maximum. Now this is the moment about G of the mass at N, and we have therefore to find the maximum value of this moment. It obviously has its maximum value when the mass at M is zero. In this case—

$$\frac{1}{f_1} + \frac{1}{f_3} = \frac{1}{F}\,; \text{ C max.} = \frac{V_1}{f_1} = \frac{V_3}{f_3}$$

And therefore—

$$\text{C max.} = \frac{V - V_2}{F} \cdot \frac{V + V_2}{2V} \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Similarly, if C min. be the smallest value that the greatest of the earth currents can have in any given case, then—

$$\text{C min.} = \frac{V - V_2}{F} \cdot \frac{V_2}{V} \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

Again, since P the leakage watts expended in earth currents is given by the equation—

$$P = \frac{V^2 - V_2^2}{F} - \frac{V^2}{f_2}$$

$$\text{P max.} = \frac{V^2 - V_2^2}{F} = 2\,V\,\text{C max.} \quad . \quad . \quad . \quad . \quad (3)$$

And—

$$\text{P min.} = \frac{V_2(V - V_2)}{F} = V\,\text{C min.} \quad . \quad . \quad . \quad (4)$$

Suppose now that we earth the middle wire of this system through a resistance of x ohms. The new potential V'_2 of the middle wire will be given by—

$$V'_2 = V_2 \frac{x}{F + x} \quad . \quad . \quad . \quad . \quad . \quad (5)$$

And it is easy to show that,

$$C \text{ max.} = \left(V - V'_2\right) \frac{V + V_2}{2\,V\,F}$$

$$= \left(V - \frac{x}{F + x} V_2\right) \frac{V + V_2}{2\,V\,F} \quad . \quad . \quad . \quad . \quad (6)$$

$$C \text{ min.} = \left(V - \frac{x}{F + x} V_2\right) \frac{V_2}{V\,F} \quad . \quad . \quad . \quad . \quad . \quad (7)$$

$$P \text{ max.} = \frac{V^2}{F} - \frac{x}{F + x} \frac{V_2^2}{F} \quad . \quad . \quad . \quad . \quad . \quad (8)$$

$$P \text{ min.} = \frac{V\,V_2}{F} - \frac{x}{F + x} \frac{V_2^2}{F} \quad . \quad . \quad . \quad . \quad . \quad (9)$$

In the particular case when the middle wire is dead-earthed, then—

$$C \text{ max.} = \frac{V + V_2}{2\,F} \quad . \quad . \quad . \quad . \quad . \quad (10)$$

$$C \text{ min.} = \frac{V_2}{F} \quad . \quad . \quad . \quad . \quad . \quad (11)$$

$$P \text{ max.} = \frac{V^2}{F} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

$$P \text{ min.} = \frac{V\,V_2}{F} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

The above formulæ show that whether we earth the middle wire or not, $\frac{V + V_2}{2\,F}$ and *a fortiori* $\frac{V}{F}$ is a superior limit to an earth current, and that $\frac{V^2}{F}$ is a superior limit to the power expended in earth-leakage currents. This latter result is known and is proved in another manner in a paper by the author on "Insulation Resistance and Leakage Currents," which appeared in the *Electrician*, vol. 41, p. 206.

The following numerical examples show how these results can be applied in practice :—

(1) The maximum output of a three-wire direct-current station with 200 volts between the outers is 3,000 kws. What is the lowest insulation resistance which would ensure that no earth-leak- is greater than the thousandth

part of the maximum supply current, the potential of the middle main being 20 volts ?

In this case $V = 100$, $V_2 = 20$, the maximum current of supply is 15,000 amperes, and therefore—

$$C \text{ max.} = 15.$$

Substituting these values in formula (1)—

$$15 = \frac{80}{F} \cdot \frac{120}{200};$$
$$\therefore F = 3{\cdot}2 \text{ ohms.}$$

And from (3)—

$$P \text{ max.} = 3 \text{ kws.}$$

Suppose now that the middle wire was dead-earthed, then from (10)—

$$15 = \frac{100 + 20}{2F};$$
$$\therefore F = 4 \text{ ohms.}$$

And from (12)—

$$P \text{ max.} = 2{\cdot}5 \text{ kws.}$$

(2) Suppose now that the pressure was increased so that the P.D. between the outers was 500 volts, what must the insulation resistance be in this case ?

Supposing that the output is the same as in the last case, then since the voltage has been increased $2\frac{1}{2}$ times the maximum earth current must not be greater than 6 amperes, and F must be $(2{\cdot}5)^2$ times as great as in the last example, *i.e.*, 20 ohms. When the middle wire is dead-earthed, then F must be greater than 25 ohms.

In practice, however, the maximum output would be increased 6·25 times, and the maximum permissible earth current would therefore be 37·5 amperes. Hence the insulation resistance of the network need only be the same as when there was 200 volts between the outers. The lowest permissible value of "F" would therefore be 3·2 ohms, and when the middle was earthed 4 ohms. Most electricians would regard these values as too low.

(3) The potentials of the mains of a three-wire direct-current system are 300, 100, and -100 volts respectively,

[illegible]

$$= 5{\cdot}33 \text{ amperes.}$$

(?) The potentials of the mains of a three-wire direct-current distributing system are 105, 85, and -20 volts respectively, and the insulation resistance is 2·5 ohms. Find the limits between which the earth currents to the negative main must lie, and also the limits for the leakage power.

By the formula

$$C \max. = \frac{105^2 - 85^2}{210 \times 2{\cdot}5} = 7{\cdot}24 \text{ amperes.}$$

$$C \min. = \frac{85 \times 20}{105 \times 2{\cdot}5} = 6{\cdot}48 \quad ,,$$

Whatever the values of the fault resistances may be, the leakage current to the negative main lies in value between 6·48 and 7·24 amperes.

Also, P max. = 1·52 kws. and P min. = 0·68 kws.

If we were to earth the middle main, then the current to the negative main would lie between 38 and 34 amperes, and the power expended in leakage earth currents would lie between 4·41 and 3·57 kws. The current in the earth connection also would be 34 amperes. In this case there would be obviously nothing to gain by earthing the middle wire. On the contrary, the current to the negative would now be doing five times the amount of electrolytic damage, and we should be wasting about three kilowatts all the year round.

The Board of Trade Regulation (A. 8) was framed to meet the danger of fire-risk, and is a scientific rule founded on certain assumptions as to the laws that govern the progress of electric lighting. When we are considering three-wire or five-wire systems, however, it is difficult to apply it, and the raising of the pressure of the supply and the enormous growth of supply networks have brought other considerations to the front. The danger to the public now lies rather from possible shocks or from the corrosion of pipes by leakage currents. The rule for the insulation resistance of the various private installations looks after the fire-risk.

It might be advisable in the best interests of the electric lighting industry to have a return taken of the insulation resistance of all the three-wire networks in this country. By what has been shown above, the mere measurement of V_2 and F need only take a few minutes, and can be done from any point on the network when the earth connection from the middle main is removed. With the help of the formulæ given above, a simple rule might then be devised which would safeguard the interests of the public and be acceptable to station engineers.

Summary.—The following are the most important points in this paper :—

(1) A graphical method of finding the potentials of the mains from their fault resistances. This construction simplifies the proof of many important theorems.

(2) A simple method of measuring insulation resistance.

(3) A discussion of various ways of keeping down the potential of the positive outer. All of these methods, however, effect this by increasing the leakage currents.

(4) When the middle main is earthed through a connection, it ought always to be removable, so that the insulation resistance could be accurately found.

(5) A discussion of the allowable leakage currents. Knowing V, V_2, and F, it is shown that the maximum earth current must lie between certain limits. These limits and the limits for the leakage watts expended in earth currents are given when the middle wire is earthed through a resistance of x ohms.

(6) When the potential of the positive outer is above 250 volts, then the higher the insulation resistance of the system the easier it is to keep it down to 250 volts. If it be required to reduce the potential of the outer by v volts, then if we connect the middle to earth through a resistance of $\frac{V_2 - v}{v}F$ ohms, this will be done, and the current in this earth connection will be $\frac{v}{F}$ amperes.

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The Three Hundred and Fifty-sixth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, January 10th, 1901—Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on December 20th, 1900, were read and confirmed.

The Secretary read the following letter from Colonel Crompton :—

"THRIPLANDS,
"KENSINGTON COURT,
"W.
"*December 28th*, 1900.

"The Secretary,
"Institution of Electrical Engineers,
"28, Victoria Street,
"Westminster.

"SIR,

"I have the honour to acknowledge the receipt of your letter of December 20th, in which you inform me of the Resolution proposed by Mr. Alex. Siemens, and carried by the Institution, congratulating the Corps of Electrical Engineers on their South African services. I will take steps to ensure that every member of the corps is acquainted with the kind action of the Institution in this matter.

"In return, on behalf of the Officers, Non-Commissioned Officers, and Sappers of the corps, I beg to thank the Institution, not only for their kind expressions of sympathy with us, but also for all they have done for us in the past, which has been of most material assistance to us in rendering our services of use to the country.

"Very faithfully yours,
"R. E. CROMPTON,
"Lt.-Col., E.E."

The names of new candidates for election into the Insti-

tution were announced, and it was ordered that the list should be suspended in the Library.

The following transfers were announced as having been approved by the Council :—

From the class of Associate Members to that of Members—

Frederick William Anthony Knight.
William Stevenson.
Stephen John Watson.

From the class of Associates to that of Members—

Henry Walter Jenvey.

From the class of Associates to that of Associate Members—

Gerald Ricketts Blackburn.
William Brew.
Arthur Blackburn Child.
Julian A. Halford.
Gilbert J. Lloyd.
Cyril Napier Probyn Raikes.
Noel Burn Rosher.
R. J. Jocelyn Swan.
Robert Naudin Tweedy.
John Walton.
Richard Wightman.

From the class of Students to that of Associates—

John Tiernay Callaghan.
Fred Leslie Cruikshanks.
Mostyn R. Gardner.
Benjamin Handley.
Frank John Hawkins.
Bernard Hopps.
Elias Marcuson.
Francis Samuel Miller.
John Leslie Morris.
Frederick Walter Purse.
W. A. Quennell.
C. T. Stephenson.
Clement Erskine Vines.
Andrew Cyril Weber.

Donations to the Library, Building Fund, and Benevolent Fund were announced as having been received since the last meeting :—To the *Library* from Mr. H. W. Lindley, Mr. E. G. Mawbey, and Mr. H. W. Jenvey, Associate. To the *Building Fund* from Messrs. W. Duddell, John Rann, Capt. Jackson, J. E. Stewart, J. F. C. Snell, C. Bauer, A. E. Levin, Alfred Hay, Thomas Mills, Dr. du Riche Preller, R. A. Dawbarn, A. Rutherford, K. W. Fiddian, S. Roget, W. H. Shephard, A. Stroh, A. A. C. Swinton, H. W. Young, F. A. Glover, Joshua Shaw, E. Percy Harvey, Francis Milton, F. [illegible] F. H. Nicholson, J. M. Donaldson,

Algernon Burton, W. M. Rolph, W. Sankey, W. Golledge, L. R. Morshead, John T. Haynes, H. J. Humphreys, Frank S. Miller, A. G. Jeffreys, W. J. Cooper, A. B. Crinks, J. M. Smyth, A. C. Makovski, L. Redmayne, E. D. Morgan. To the *Benevolent Fund* from Sir David Salomons, and Messrs. A. E. Levin, O. M. Andrews, S. Roget, A. Stroh, E. Percy Harvey, W. G. Shee, Killingworth Hedges; to whom the thanks of the meeting were duly accorded.

Messrs. F. H. Varley and G. C. Allingham were appointed scrutineers of the ballot for the election of new members.

The President announced that the following had been appointed the Committee of the Birmingham Local Section for the ensuing year:

OFFICERS AND COMMITTEE OF THE BIRMINGHAM LOCAL SECTION.

Chairman: Professor Oliver Lodge, D.Sc., F.R.S.
Vice-Chairman: Henry Lea.

Committee:

F. Brown.	Capt. H. R. Sankey.
A. Coleman.	W. E. Sumpner, D.Sc.
A. Dickinson.	R. Threlfall, F.R.S.
G. S. Ram.	J. C. Vaudrey.

W. Wyld.

Hon. Secretary: D. K. Morris, Ph.D.

The PRESIDENT: I have to announce that the Inaugural Meeting of the Birmingham Section will take place on Wednesday, January 23rd. Your President and the Secretary will, of course, attend that meeting. I also announce to you the formation of our first Local Section in India, the Calcutta Local Section of the Institution of Electrical Engineers.

It was, further, announced that the Council had agreed that, for the present, the areas of three local sections should be as follows:—

Birmingham: The counties of Staffordshire, Warwickshire, and Worcestershire.

Dublin: An area defined by a circle with a radius of twenty-five miles from Dublin (with the understanding that in special cases where a more distant

member proposes to attend the meetings of the Section, such cases should be specially considered by the Committee of the Section, and the member claimed if it is thought right).

Newcastle-on-Tyne: The counties of Northumberland, Durham, and Cumberland, with the town of Middlesbrough and Cleveland District.

The following paper was then read :—

THE USE OF ALUMINIUM AS AN ELECTRICAL CONDUCTOR, WITH NEW OBSERVATIONS UPON THE DURABILITY OF ALUMINIUM AND OTHER METALS UNDER ATMOSPHERIC EXPOSURE.

By JOHN B. C. KERSHAW, F.I.C.

I. INTRODUCTION.

The high price to which copper has been forced in recent years by a combination of natural and artificial causes—a combination which it is unnecessary to discuss in this paper—has led to a renewed interest in aluminium as a substitute for copper in electrical power transmission schemes.

Ten years ago the price of aluminium was 8s. 4d. per lb., and its use as a substitute for copper for electrical purposes even with the latter metal at £80 per ton, was therefore economically impossible. The purity of the metal manufactured in the early days of the electro-metallurgical process, by which the whole of the supply of the metal is now produced, was also variable; and it is only within the last few years that the improvements in the process of manufacture, in their relation to the quality and price of the metal produced, have brought aluminium within the practical range of the electrical engineer as a substitute for copper.

The following table shows that the price of aluminium has fallen and the quality has improved with increasing output, each year since 1890, when the present electro-

metallurgical process of manufacture was first adopted in this country.

TABLE I.

WORLD PRODUCTION AND AVERAGE PRICE OF ALUMINIUM, EACH YEAR FOR THE PERIOD 1890–1900.

Year.	Production in Metric Tons of 2,204 lbs.[1]	Price in Pence per lb. in U.S.A.	Quality.[2]
1890	165	...	
1891	233	75	
1892	487	49	
1893	715	37	·93 to 1·64 % Silicon, ·32 to 1·66 % Iron
1894	1,240	30	
1895	1,418	27½	
1896	1,789	20	
1897	3,394	17½	
1898	4,033	16⅕	·02 to 13 % Silicon, ·12 to ·32 % Iron
1899	5,000 (Estimated)	16⅕	

II. RELATIVE COSTS OF COPPER AND ALUMINIUM.

In order to compare the price of aluminium with that of copper when used for conducting purposes, it is necessary to make allowance for the fact that the Specific Gravity of aluminium is rather less than one-third that of copper, and that its conductivity for wires of equal sectional area is only from 50–63 per cent. that of the more common metal. The following formula is useful for calculating the relative prices of equal lengths of bare conductors of the two metals, of equal electrical capacity :—

$$\frac{S \times P \times c}{s \times p \times C}$$

[1] From *The Mineral Industry*, vol. viii., 1900.
[2] Moissan's tests, *Comptes Rendus*, 1898.

[illegible] represent the specific Gravity, Price, and [illegible] represent the [illegible] aluminium.

[illegible] physical constants [illegible] market prices, [illegible]:—

[illegible] per ton

and [illegible] given above, we obtain [illegible]:—

[illegible]

[illegible] expended [illegible] aluminium [illegible] carrying capacity; and [illegible] the dearer metal of the two.

[illegible] where large quantities [illegible] wire [illegible] for electrical purposes, [illegible] United States large quantities of the new metal have been sold at [illegible] per lb. = £135 per ton.[1]

Using this figure in the formula given above, the cost ratio for conductors of equal length and equal carrying capacity becomes—

$$\frac{8.93 \times [illegible] \times [illegible]}{2.68 \times 135 \times 100} = \frac{1325}{1000} = \frac{Cu}{Al}$$

and copper is seen to be the more costly material.

III. Installations of Aluminium in the United States and in the United Kingdom.

The low price at which aluminium is being sold for conducting purposes in America therefore explains the

[1] Messrs. T. Bolton & Sons inform the writer that in this country £170 per ton is quoted for [illegible] the wire.

readiness of electrical engineers in that country to adopt the new metal. It may be explained here that at present there is no talk of using aluminium for insulated conductors; the greater sectional area of the metal for equal carrying capacity (1·68 : 1·00) rendering it impossible to use it for such covered conductors, until it has fallen to a much lower price relative to copper.

The following are the particulars of some of the bare aluminium transmission lines already completed across the Atlantic.

At Niagara Falls there are two aluminium transmission lines; the one conveying current from the generating station to No. 2 Works of the Pittsburg Reduction Company, and the other conveying current to the Chlorate Works of the National Electrolytic Company. Both these lines are short, and are stated to be working satisfactorily. Together they transmit 4,000 H.P.

The Hartford Electric Light and Power Company have an aluminium line between their generating station at Tariffville, and Hartford—a distance of 11 miles. About 2,000 H.P. is transmitted at 10,000 volts pressure by the three-phase system, over this line. The diameter of the stranded cable used is ¾ inch, and it weighs about 1,500 lbs. per mile.

The aluminium transmission line of the Snoqualmie Falls Power Company has been frequently described in the technical press. It runs between the Falls and the two towns of Tacoma and Seattle. Its total length is 34 miles. When the scheme is completed, 10,000 to 12,000 H.P. will be transmitted by this line at a pressure of 29,000 volts. The aluminium used has been alloyed with 1½ per cent. of copper, and the increased tensile strength of this alloy has enabled spans of 120–150 feet to be safely used.

The Blue Lakes Power Company have an aluminium line in use between their power-house at Blue Lakes, and Stockton—a distance of 36 miles; 1,000 H.P. being transmitted by the three-phase system at a pressure of 25,000 volts. The line originally erected has been replaced by one of greater carrying capacity, and 1,000,000 lbs. (446 tons) of the metal have been used for the new line. At 29 cents per lb. this represents an outlay of £60,400 (or £1,677 per mile) for the metal alone.

[illegible] 200 H.P. [illegible]

[illegible] connection [illegible]

[illegible] 200 H.P. [illegible] miles [illegible]

[illegible] 1,000 [illegible] generating station [illegible]

[illegible] Company [illegible] scheme of supplying [illegible] generating station in the [illegible] distant. The [illegible] the possibility [illegible] pressure of [illegible] use aluminium [illegible] estimates have been [illegible]

[illegible] power-transmission lines named [illegible] used in place of copper for [illegible] the Waxanachie Electric Light [illegible]; by the North-Western Elevated Rail[illegible], of Chicago; by the Kansas City and [illegible] Electric Railroad Company; and by the [illegible] Elevated Railroad Company, of New York. [illegible] purposes it is in use by the [illegible] Railroad Company, by the Pacific States [illegible] Company, and by the New York [illegible] country the Northallerton

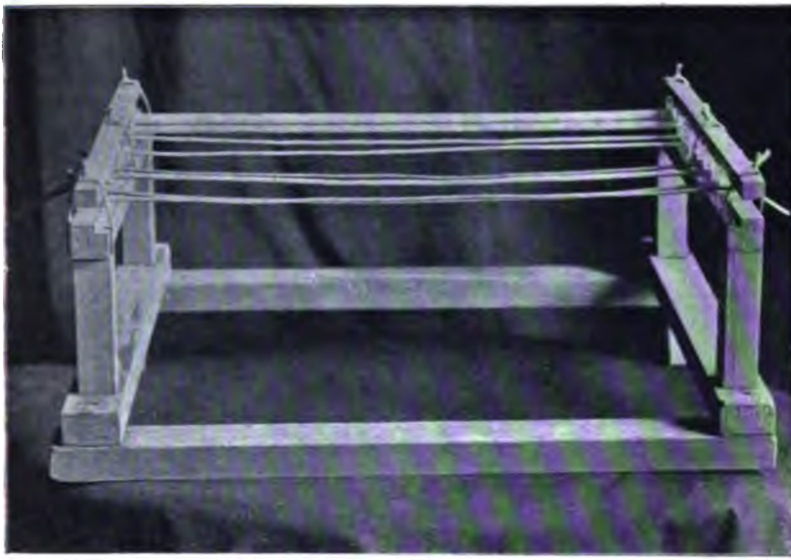


FIG. 1.—Frame used for Wire Exposure Tests.

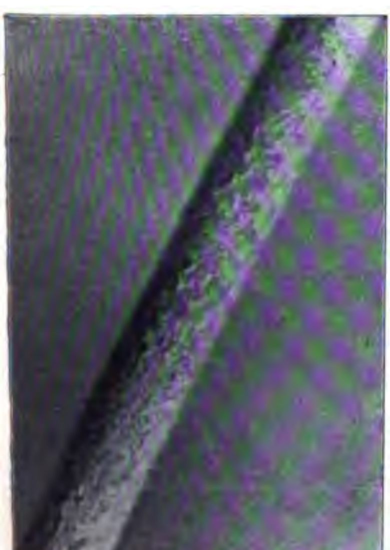
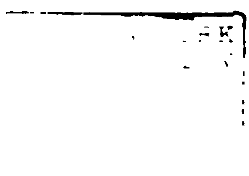


FIG. 2.—Corrosions on Surface of Aluminium Wire. Magnified 4 times.

FIG. 3.—Corrosions on Surface of Al Wire. Magnified 20 tim



Electric Lighting Company have four miles, and the British Aluminium Company, at their Foyers Works, have several miles of aluminium in use for various purposes; and the Post Office authorities are making experimental trials of the metal for long-distance telephonic communication.

The difficulty of soldering aluminium has been surmounted in most of these schemes by using mechanical joints, the MacIntyre sleeve-joint being that usually adopted. In one or two cases, as at Niagara and at Northallerton, soldered-joints have been made, but the writer doubts whether these will prove as satisfactory.

This summary of the chief installations of aluminium as an electrical conductor in the United States and in this country, shows that considerable progress has been made. In time the metal, *if it be found to possess the requisite durability*, may become an important rival of copper in this new field of usefulness.

IV. Durability of Aluminium and other Metals under Atmospheric Exposure.

The old idea that aluminium was absolutely proof against attack by all agents excepting alkalies and hydrochloric acid is now known to be incorrect; and Ditte in communications made to the Academie des Sciences[1] has shown that aluminium is easily attacked and corroded by air and water under certain conditions. It is therefore of considerable importance to inquire whether these conditions are present when aluminium is used as a bare conductor for overhead lines, and is exposed to all the varieties of weather that we experience in the course of the four seasons of the year. In America electrical engineers are conducting this inquiry in the usual American fashion; and their installations of aluminium for transmission lines in California and other places, are in reality large scale experiments which, if unsuccessful, will cause heavy losses to fall upon those who have been financing these undertakings.

In order to obtain independent and reliable figures concerning the effects of exposure upon aluminium, the

[1] *Comptes Rendus*, March–April, 1899.

writer, since October, 1899, has been carrying out two series of observations in the North of England. The samples of aluminium used are in rod and wire form, and were kindly placed in the writer's hands by Messrs. T. Bolton & Sons, of Oakamore Wire Works, and by the British Aluminium Company. In order to make the investigation more complete, the inquiry was extended to all metals and alloys used for electrical conductors, and samples of copper, tinned copper, and of galvanised iron wire were also sent to the writer by Messrs. T. Bolton & Sons. It is a curious fact that there would appear to have been no previous scientific observations upon the durability of these metals or alloys under atmospheric exposure; and the choice of the metal or alloy for overhead wires in any particular district has apparently been settled by custom rather than by scientific knowledge.

The methods of observation adopted by the writer in his experiments were as follows :—

The rods and wire were cut into 2-feet lengths, and were mounted upon glass insulators in two frames, so that their position was parallel one to the other, and horizontal with regard to the ground.

Fig. 1 is a reproduction of a photograph of one of these frames containing the sample wires. The frames carrying the wires were so placed that the drops of water collecting upon the wires in wet weather could not by any chance pass from one wire to another, and thus bring electrolytic action into play. Each length of wire was carefully marked and weighed before commencing the exposure, and the weights recorded. The one frame with its nine insulated sample wires was exposed upon the roof of a building in St. Helens, Lancashire, from October 11th, 1899 to August 23rd, 1900; and the second frame was similarly exposed at Waterloo, Lancashire, for the same period of ten months.

The climate of St. Helens is probably too well known to need description; but it has improved considerably in recent years, owing to the closing of several chemical works. The place in which the wires were exposed is now singularly free from chlorine and hydrochloric acid gases.

Waterloo is on the Mersey, six miles north of Liverpool, and its atmosphere is that of an ordinary west-country seaside town, p[illegible] d. During the exposure period

of ten months the frames were not touched or moved. At the end of this period the wires were removed from the frames, cleaned from the soot and dirt of all kinds which had accumulated upon them, and after thoroughly air-drying, were re-weighed. The results are set forth in Table II.

TABLE II.

RESULTS OF EXPOSURE TESTS OF ALUMINIUM AND OTHER WIRES.

Composition and Form of Sample.	Waterloo Set; 10 Months. + = Gain, − = Loss in Weight.	Waterloo Set; 10 Months. Remarks.	St. Helens Set; 10 Months. + = Gain, − = Loss in Weight.	St. Helens Set; 10 Months. Remarks.
	Per cent.		Per cent.	
Aluminium Rod No. 1	nil	These 5 samples were all pitted, especially on the under sides, where water-drops had collected and dried.	+ ·27	These 5 samples were very badly pitted. Dirt had settled in their corrosions and could not be removed by scrubbing.
„ „ No. 2	+ ·13		+ ·51	
Aluminium Wire No. 1......	+ ·41		+ ·83	
„ „ No. 2......	nil		+ ·83	
„ „ No. 3......	+ ·55		+ ·54	
Galvanised Iron Wire No. 1	− ·15	No change in appearance to the eye.	− 1·44	Badly corroded. Zinc partly eaten away.
„ „ No. 2	− ·16		− 2·13	
Copper Wire No. 1	nil	Oxidised on surface, but not pitted or corroded.	− 1·65	These wires were perfectly black, and could not be distinguished.
Tinned Copper Wire No. 1	nil		− 1·31	

The above figures show that the aluminium wires and rods had nearly all gained in weight during the ten months' exposure, the gain varying from nil up to ·83 per cent. on the weight of the original wire. This gain must be attributed to the corrosion of the rods and wires, and to the settling of soot and dirt in the crevices. No amount of scrubbing would remove this dirt. The weights of these aluminium rods and wires were therefore of very little use in determining how far they had suffered by the exposure, and the two samples of aluminium wire marked No. 1 in Table II. were submitted to conductivity, and tensile strength, tests.[1] The results are given in Table III.

[1] These tests were made by the Faraday House Testing Institution.

TABLE III.

TESTS OF ALUMINIUM WIRE FOR CONDUCTIVITY AND TENSILE STRENGTH.

	Conductivity: Copper 100.	Tensile Strength: tons per square inch.
Original wire	51·3	13–16[1]
Waterloo sample	51·4	12·06
St. Helens sample	49·0	11·15

It has been customary to assume that the aluminium wire supplied for electrical purposes had a conductivity of from 57–63 per cent. that of copper, taking equal sectional areas of the two metals. The following figures were in fact sent to the writer with the samples of aluminium wire used in some of these experiments.

Sample.	Composition.	Conductivity: Copper 100.	Tensile Strength: tons per square inch.
1. Rod	98·004 per cent. Al.	60–62	16–18 tons
2. Wire	99 per cent. Al.	65	13–16 tons
3. Wire	99 per cent. Al. 1 per cent. Fe.	62	16–18 tons

Samples 2 and 3 were, however, submitted to independent tests for conductivity, before exposing, and it was found that in place of conductivities of 65 and 62 per cent., they only possessed conductivities of 51 per cent. and 54 per cent. respectively. It is, of course, known that pure aluminium has a conductivity 63 per cent. that of copper; and the low conductivity of these samples can only be explained by the presence of iron, or other metals, introduced to increase their strength. The conductivity tests supplied by the firm in question were evidently based on surmise, not on actual results.

The tests given in Table III. show that although the St.

[1] This test was supplied by the firm from whom the wire was obtained, and it is there[illegible] comparable with the others.

Helens wire had gained in weight, yet considerable loss in conductivity and tensile strength had resulted from the exposure and consequent pittings. The Waterloo samples, although pitted to a less extent, had not lost in conductivity; and the tensile strength had probably not suffered, although in the absence of a special test of the original wire it is impossible to be quite certain on this point. The remains of these two sample wires are here on the table for examination by those interested in the subject, and photographs of the more badly corroded wire are also exhibited.

Examining Table II. again, in order to study the results obtained with the remaining wires, we see that both in Waterloo and in St. Helens the galvanised iron wires had lost in weight; the losses in the latter case being serious and amounting to 1·44 per cent. and 2·13 per cent. of the original weight of the wires. In this case almost the whole of the zinc had been dissolved away by the action of the acid gases, and the exposed iron was badly oxidised. The two Waterloo samples were, on the other hand, bright and clean, and to the eye did not appear to have suffered.

The copper and tinned copper wires exposed at Waterloo were oxidised on the surface; but no pitting had occurred, and there was neither loss nor gain in weight. The two samples of similar wire exposed at St. Helens had both lost in weight (1·65 and 1·31 per cent. respectively), and as the whole of the tin had been dissolved off the tinned copper wire, it was impossible to distinguish one from the other.

V. Conclusions.

It is perhaps unwise to found any general conclusions upon the results of these observations, since they refer to two districts only. The British Isles can afford a very wide and ample selection in the way of climates. It would, of course, be interesting to have similar series of observations established in London, and in one or more of our large cities with a manufacturing population—say Manchester, Glasgow, or Sheffield; but at the moment the writer is unable to establish these. The investigation, however, proves that the aluminium wires at present sold for conducting purposes in this country are not perfectly resistent to atmospheric corrosion, and that in the atmosphere of a town

[illegible] in the air [illegible]

[illegible] town, it is [illegible] large amount of [illegible] purposes, the [illegible] obtaining there, [illegible] somewhat [illegible]

[illegible] overhead con- [illegible] towns [illegible] and the pro- [illegible]

[illegible] where the [illegible] minimum, a [illegible] but the [illegible] such an [illegible] the neigh- [illegible] may in time lead [illegible]

[illegible]

[illegible] he may [illegible] the subject [illegible]

[illegible] which [illegible] the Director [illegible]

[illegible]

[illegible] meeting of the Insti- [illegible] I noticed [illegible] Mr. [illegible] that it might be thought [illegible] interesting observations on [illegible] aluminium and other metals for certain elec- [illegible] purposes. If this is the case, I shall be glad to endeavour to

[ar]range to have such experiments carried out at the Natural Physical [L]aboratory, and shall welcome the advice and assistance of any [m]embers of the Institution interested in the matter.

"Yours truly,

"R. T. GLAZEBROOK."

I believe I have heard that Lord Kelvin is making pretty [l]arge experiments on this subject, and that he has spread [s]ome aluminium wires to find out their durability some[w]here in Scotland—at Foyers, I believe.

Mr. J. GAVEY: The Post Office has, for about twelve months, had under consideration the desirability of carrying out certain experiments with aluminium wire, and the reason why they were deferred for such a long period was the difficulty in obtaining a suitable soldered joint. Old telegraph engineers, whose memories carry them back to the period before the invention of the Britannic joint, have a great horror of unsoldered joints; and before we undertook to carry out experiments we deferred our trials, at all events until something in the nature of a satisfactory soldered joint could be obtained. The question of soldering seemed to be solved so far as the mere soldering of joints was concerned, but unfortunately the heat that was necessary for the soldering annealed the wire and reduced its strength by exactly 50 per cent. Ultimately, not to delay the experiments any longer, we arranged to run out the wires in the full length of the coil, to erect each coil of wire on an insulator, and to solder the joint in a little loop, so that the annealing of the wire itself had no injurious effect on the strength of the portion that was suspended. We have now obtained something like 18 or 20 miles, and about 15 miles of this wire were erected in the Potteries, near Hanley. We thought that was a very suitable place to expose it for a crucial trial. The wire handled very well, but our first experiment has not been very promising. We erected 15 miles of the wire early in December, and within about a fortnight of that time a heavy gale of wind blew across the country, and there were about eight or ten breaks in these spans that were erected. The wire that was put up weighs 75 lbs. to the mile; its approximate diameter is 1·24 millimetres; its breaking strain was 340 lbs., which, being reduced to tons per square inch, came out at 12·568 tons; it stood ten twists in three inches; its maximum resistance per mile was 6·158 ohms, and its specific resistance came out at 2·974 ohms. I do not want to make too much of this, and it is perhaps a little too early to come to any conclusion on the subject, beyond, perhaps, this: that it is quite possible that, either in the preparation of the billets or in the rolling or drawing of the wires, there were places which were either very crystalline or which differed in character from the remainder of the wire. I think, carrying back our experience to the original use of hard-drawn copper wire for telegraph and telephone circuits, we met with considerable difficulties in obtaining exactly the quality of wire that we asked for. It was very difficult, in the first place, to meet the rather stringent specifications of the Post Mr. Gavey.

Mr. Gavey. Office, but our English manufacturers—and I am glad to say this, after all the obloquy that has been heaped upon them—came to the front, and, after considerable expenditure, they did succeed in meeting our requirements. No doubt in the manufacture of aluminium wire, when a little more experience has been obtained, similar success will attend the efforts of manufacturers. You may take it for what it is worth, that the first trials have not been quite so satisfactory as we hoped for. I should have added, perhaps, reference having been made to San Francisco, or rather to California, that I had the pleasure of a conversation with Mr. Sabine, the President of the Pacific Telephone and Telegraph Company, about six months ago. He had erected a considerable length of aluminium wire for telephonic purposes, and he told me that the line had been subjected to breaks of a somewhat similar character to those that we have experienced, and for which they could not exactly account.

Mr. Swinburne. Mr. SWINBURNE: In this paper there is one very important point which is not very fully dealt with, and that is the question of the purity of the aluminium. A very small portion of any foreign metal in aluminium will probably lower the conductivity, and therefore throw the price-ratio wrong; that is to say, a very little further purification of the aluminium will probably make its current-carrying power greater, so that the price-comparison is altered. A very little further purification will also probably prevent the corrosion. The three chief impurities in aluminium are sodium, which is about the worst element you possibly can have, from the corrosion point of view; silicon, which probably also gives rise to corrosion; and iron, which perhaps does not matter, except as to resistance. The manufacturers of aluminium are continually improving it, and therefore there is considerable hope in that direction. But the next difficulty, which Mr. Gavey has also mentioned, was the question of soldering. Nearly all the aluminium alloys are unstable if they have much aluminium; that is to say, the aluminium alloys with a lot of the other metal and a little aluminium are generally good; also with a lot of aluminium and a little of the other metal they are generally good. But the aluminium alloys with about equal proportions of one metal and aluminium are very curious; they are nearly always unstable, and very many of them tumble to powder without being touched. It is quite easy to solder aluminium, but the solder does not necessarily last more than two or three months, which is a most annoying thing. It has struck me—I have been told that I am wrong, but I have never been told why—that there is a practical difficulty if you use a solder that is not nearly all aluminium, because if you use any other solder there must be a surface cutting the joint somewhere that is in the unstable condition, and that surface is probably what breaks. Mr. Kershaw says that he thinks aluminium is hopeless for insulated conductors. I do not think that is altogether so. It rather bears, perhaps, on the next paper, but it may be interesting to point out what I mean now. Aluminium is chiefly used for overhead conductors. When you get a very high pressure on an overhead conductor, you must remember that the slope of the potential varies as the distance from the centre, so that it is very great just near the surface of the

metal, and the higher the potential and the smaller the lead the greater the fall. The result is that when you get up above 20,000 or 30,000 volts with a small conductor, if you look at it in the dark you will see discharges going on all over it. The air is breaking down, and is getting, as it were, torn. That means waste of power, and these leaks for very high pressures become very serious. Therefore aluminium is better than copper, unless you make the copper into a tube. It is probable that it will be cheaper to use, considering how near they are in price, a big aluminium wire than a copper tube. The same thing comes in when you are dealing with insulated conductors for high pressures. It may quite happen with a large power-transmission that either a copper tube must be used for the central, or else aluminium; and in that case you have no further chance of corrosion. It is quite possible that aluminium may be exceedingly important, even in covered conductors, in large engineering schemes. Mr. Swinburne.

Professor George Forbes (*communicated*): In reference to Mr. Kershaw's paper on the use of aluminium as an electric conductor, I beg to quote the following passage from a letter written to me by the vice-president and treasurer of the Niagara Falls Power Company, and dated Niagara Falls, N.Y., December 29, 1900: "We are building our second line to Buffalo with aluminium cables, and shall furnish 5,000 H.P. to the Pan-American Exposition beginning April 1st." Professor Forbes.

In the same connection I may mention that I have had exposed at Fettercairn, Kincardineshire, two aluminium conductors, each ½ inch diameter and 200 yards long, one for eighteen months and the other nearly three years—the one being of aluminium from Neuhausen, the other from Foyers. Both of these are in a perfectly good condition.

Mr. E. Ristori (*communicated*): The Company in which I am interested is, of course, quite as anxious as any one else to prove conclusively whether aluminium can advantageously be adopted as an electrical conductor, and on this point we may remark that during the last three or four years, the Company has had several miles of aluminium wire and strip working for telephone, lighting, and power transmission purposes at Foyers, and that up to the present, we have not had any trouble with these installations, either electrically, on account of the conductivity being lower than that calculated for, or mechanically, from breakdowns due to the weather, which is not of the best in that locality. Moreover, I should mention that this plant was not erected in the nature of an experiment, as we were already convinced of the suitability of aluminium for the purposes required, but for permanent and heavy work, and, as I say, the results have been in every way satisfactory. The metal used was that which we are turning out at our factory, and I am arranging some experiments with a view to ascertaining the present conductivity of some of these wires, which have now been in use for several years, and I hope to communicate the results to the Institution later. Mr. Ristori.

The figures given in Mr. Kershaw's paper representing the conductivity of his specimens as being between 51 per cent. and 54 per cent. that of copper, I must say are very much below those obtained

[illegible] being produced at the [illegible] per [illegible] We [illegible] exper- [illegible] well-known fact of [illegible]

[illegible] seems [illegible]

[illegible] affected by [illegible]

[illegible] Mr. Kershaw's paper [illegible] misleading [illegible]

[illegible] the paper with [illegible] interest [illegible] both [illegible] Despite the [illegible] aluminium [illegible] from the data supplied [illegible] alone. [illegible] For covered [illegible] Taking the price of [illegible] conductivity [illegible] Mr. Kershaw [illegible] compete with its [illegible]

[illegible]

Should, however, the price of aluminium fall so that its physical properties still render it an advantageous substitute for copper. It is certain that at least in smoky atmospheres the corrosion which takes place is considerable. This is shown by the sample tested at St. Helens, which, although fairly constant in weight, lost during the trial [illegible] in conductivity: a clear indication of corrosion and waste. On the other hand, although the data given by the author relating to the copper wire is not conclusive in the absence of conductivity tests of these wires after exposure, yet the small loss in weight on the wires experimented on at St. Helens would seem to imply less corrosion than was suffered by the aluminium wires.

With [illegible] I should expect the corrosion and waste to be greater [illegible] as the dirt accumulated and the oxide formed

would probably protect the wires to some extent after long use. All these points can only be determined by a lengthened exposure and frequent tests. Mr. Gibbings.

Tinning copper wire intended for use in a bad atmosphere, especially near chemical works, is apparently a waste. This is only what might be expected, seeing that there is always present in the atmosphere of such localities hydrochloric acid and free chlorine.

In comparing the relative value of copper and aluminium for electrical conductors, it should not be overlooked that, whilst the conductivity of aluminium has a downward tendency, due to the necessity of using a strengthening alloy, the conductivity of copper is increasing by improved methods of manufacture. My company is now delivering as a regular thing copper of 101 per cent., and occasionally 102 per cent. is reached. The tensile strength is also increasing, and is now considerably above the requirements demanded by the Post Office specification.

With aluminium, the use of mechanical joints, which appear to be the most satisfactory, must, on long lines, materially affect the resistance of such line.

Mr. KERSHAW, in reply (*communicated*): I am gratified to learn from Professor Glazebrook's letter to the President of this Institution that he considers the question of the corrosion of aluminium and other wires under atmospheric exposure of sufficient importance to warrant the commencement of exposure tests at the new National Physical Laboratory at Kew. Mr. Kershaw.

I have stated in my paper that it would be interesting to have a series of observations carried out in different localities, and, I therefore, hail Professor Glazebrook's offer with satisfaction. It will be instructive to compare the results obtained at Kew, with the commercial samples of wire, with those I have obtained in Lancashire.

With regard to the President's statement that Lord Kelvin is carrying out some exposure tests with aluminium conductors in the North, I may say that I was not aware of this fact. Professor Forbes has, however, instituted some practical tests of this character in Scotland, and possibly these are the observations to which Professor Perry alludes.

Mr. Gavey, to whom I am much obliged for his information concerning the Post Office trials of aluminium, has referred to the difficulty met with in making satisfactory joints with the new metal. I suppose Mr. Gavey is aware that in America a mechanical sleeve-joint is generally adopted, and that it is giving most satisfactory results. A new method of "autogenous soldering"—or welding—has also recently been brought out and patented by Heraeus of Hanau, and at the Paris Exhibition a wire 5 feet long, made up of twelve separate pieces joined together by this method, attracted much attention. The method is fully described by the inventor in the "Zeits. f. Angew. Chemie" of July 24, 1900. The difficulty of jointing aluminium is, therefore, practically surmounted.

Mr. Swinburne in his remarks has pointed out that I have been incorrect in stating that there is no future for aluminium as an insulated

Mr. Kershaw. conductor. In my remarks upon this point I was referring of course solely to the greater surface area of conductors of the light metal, and I am ready to bow to Mr. Swinburne's superior knowledge, and to accept his statement that the cost of insulating aluminium for *high tension currents* will be less than in the case of copper.

With regard to the influence of small amounts of impurity upon the conductivity of aluminium wires, I am aware of the importance of this aspect of the subject.

My experiments were, however, directed towards finding the durability of the metal now sold for conducting purposes, and it would not have been of any practical value to confine my experiments to specially prepared and purified forms of aluminium.

I have given in my paper the "official" descriptions of three of the samples used in the experiments. The discrepancy in the conductivity tests has, however, caused me to doubt the value of these "official" descriptions; and before the results of the further observations with the wires exposed at St. Helens and Waterloo are published, I shall hope to have completed independent analyses of the various metals and alloys used in these experiments.

[*Communicated March* 15, 1901]: Mr. Ristori in his communicated remarks has thought it necessary to impugn the accuracy of the conductivity tests published in my paper. It would, however, have been more to the point if Mr. Ristori, in place of quoting general tests, had given the *actual* conductivity tests of the wire, samples of which were supplied by his firm to me in 1899. Personally I have little doubt as to the correctness of the tests given in my paper, and confirmation from an independent source will be found in the issue of the *Electrical Review* for March 8, 1901. In a report upon the condition of the aluminium wires erected at Northallerton in 1899, it is stated that the original electrical conductivity of the wire was only 51 per cent. This would seem to indicate that the wire sold by the British Aluminium Company in 1899, for electrical purposes, was far below the 60–65 per cent. conductivity standard referred to by Mr. Ristori; and some of the samples placed in my hands by the British Aluminium Company in that year were evidently similar in composition to the wire erected at Northallerton.

In conclusion I may state that my interest in the subject is purely scientific, and that I am only desirous of arriving at a true estimate of the future that awaits "aluminium" in this new field of usefulness.

The President. The PRESIDENT: I will ask you to give your thanks to Mr. Kershaw for this valuable paper.

The resolution was carried by acclamation.

The following paper was then read:—

CAPACITY IN ALTERNATE CURRENT WORKING.

By W. M. MORDEY, Member of Council.

This is an attempt to consider some of the effects of electro-static capacity in insulated cables for alternate current working, especially as regards the power and plant.

Distinctions formerly drawn between "current electricity" and "static electricity" have left impressions on our minds by no means helpful now. When we come to deal with electro-static capacity in engineering applications with rapidly alternating currents, we find that so far from being "static" the conditions and effects are those of current or flow, and may most usefully be studied as such.

Electro-static capacity in cables in some respects is an advantage, but on the whole it is a serious drawback in alternate current work, mainly because it is directly and indirectly a cause of waste of power.

Now that many applications of extra high tension alternate current distribution are being promoted, it may be useful to carefully examine what is involved in this property as regards the power and plant required.

In attempting to do this I may be allowed to begin at the beginning, as I think this may be useful to many engineers who have not hitherto considered the subject in its relation to economy of working.

The capacity of a cable is measured usually by comparing it with that of a standard condenser, using a low pressure and observing the throw of a ballistic galvanometer.

The unit of capacity is the farad, but as it is too large for practical purposes, the practical unit adopted is the microfarad or $\frac{1}{1,000,000}$ of a farad.

Electric supply cables have a capacity varying from about one-fifth of a microfarad to about one microfarad per mile, according to the size, the form, and the kind of insulating material used.

If two cables are connected to the terminals of an alternator, a current to charge the cables will flow into them as the pressure rises in each half period, and back again as the pressure falls. As this occurs usually from 100 to 200 times a second it is of course just as "continuous" as any other alternating current. The amount of this charging current is easily found if we know the capacity of the cable.

Perhaps I may be allowed to give a practical idea of what a microfarad is, in terms of volts, amperes, and periods.

... and
... the scale of the ammeter ... read
directly ... This method ... merely
according ... of the subject ... does not
appear ... in standard works on the subject.

As it can be applied wherever an alternator is available, it should be useful to station engineers and others, especially for taking the capacity after the mains have been laid. The

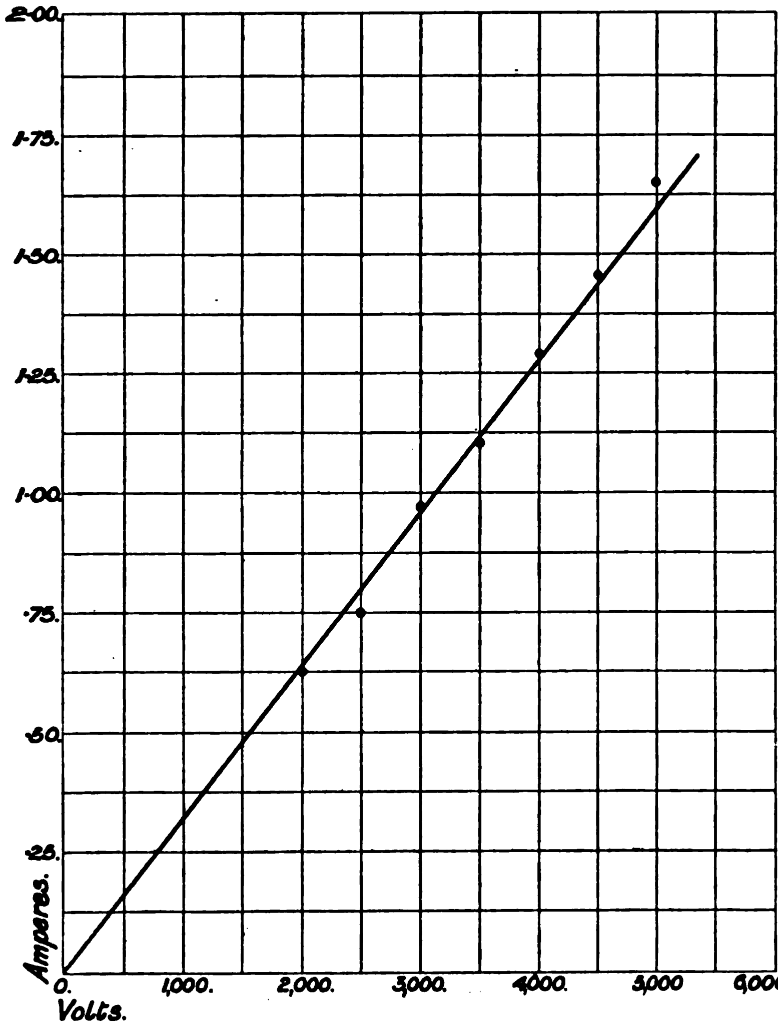


FIG. 1.—Capacity Current taken by Cable at Various E.M.F.'s, 50 ∿.

test can be made at the full working pressure—and variations of capacity with age may be easily observed.[1]

In order to show what the capacity current may amount

[1] Since reading this paper I have found this method useful only with true sine curve alternators—see discussion, p. 396.—W. M. M.

[illegible]

TABLE 1

[illegible]

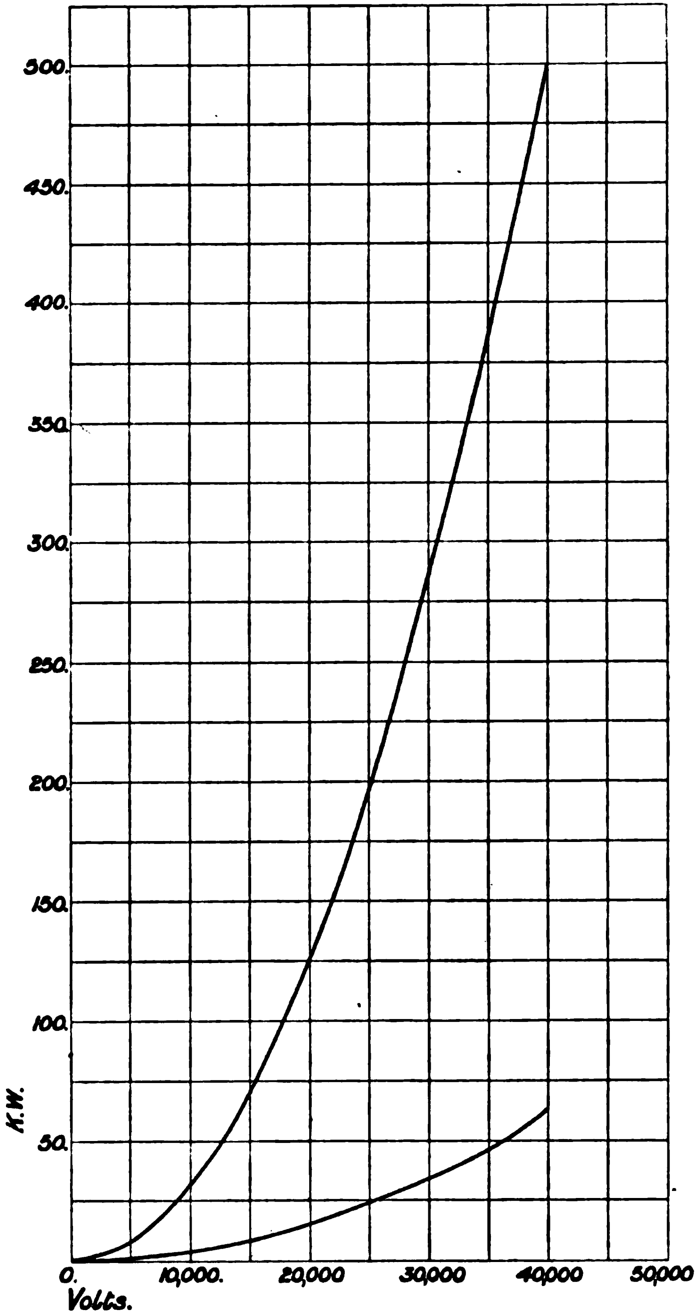


FIG. 2.—Apparent and Real K.W. taken by Cable of 1 mfd. at various E.M.F.'s 50 ∿.

Upper Curve = Apparent K.W.

Lower " = Real K.W.

Fig. 2 is plotted from this table, the upper curve giving the apparent power, and the lower curve the true power. Fig. 3 is the lower part of Fig. 2 drawn to a larger scale.

At 100 ∿ the current and watts would be double, and at 25 ∿ they would be one-half of the values given in the table.

When it is remembered that large distribution systems working at 10,000 to 20,000 volts may, and probably often will, have cables the capacity of which may be scores of microfarads, it will be realised that the magnitude of the capacity effect is considerable. Even at low and moderate pressures it is by no means negligible.

The term "apparent watts" is rather unsatisfactory. It is generally used to indicate that the whole of some volt-ampere quantity is not true watts, but it is sometimes applied to volt-amperes that are not watts at all. For instance, people will say, "So many true watts and so many apparent watts." The safest plan seems to be to use the term "apparent watts" as including both true and false watts, adding the power-factor, when known, to show how many of the apparent watts are true and how many are false.

The next question to consider is, What is the power-factor of the cables? This is very important, for whatever we may be able to accomplish in neutralising or compensating the charging current, so far as I know we can do nothing to reduce the true watts absorbed by the dielectric of any given cable at any given pressure and periodicity. These true watts are made up of ordinary copper loss or C^2R due to any charging current flowing in the conductor, to leakage, and to dielectric hysteresis.

The C^2R loss due to the passage of the capacity current is usually unimportant, at least in large cables. It is easily calculated.

The leakage is also usually unimportant. Thus if a 2,000-volt cable has an insulation resistance of say 1,000 megohms per mile, the leakage will be only 2 millionths of an ampere and 4 thousandths of a watt per mile.

Then there is the loss from dielectric hysteresis; that is, the loss of energy due to the insulating material being subjected to rapidly repeated and violent electric strains.

Whether this effect is simply due to mechanical friction caused by rapidly repeated attractions and repulsions

between the particles of the insulating material, and between that material and the metal within it and outside of

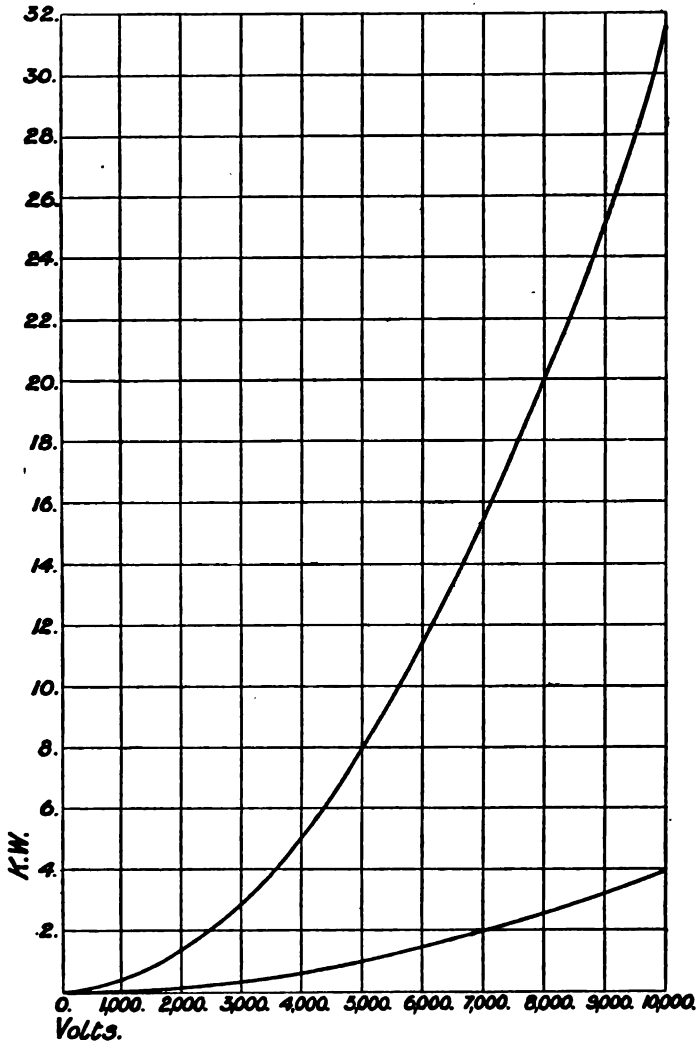


FIG. 3.—(This is the lower part of Fig. 2 drawn to a larger scale.)
Upper Curve = Apparent K.W.
Lower „ = Real K.W.

it; or whether it is due to some more obscure molecular or polarisation effect, I do not know. A simple explanation is to be preferred if a satisfactory one can be got.

[illegible]

[illegible] The [illegible]

[illegible] say, the [illegible] the apparent [illegible]

[illegible] and the leakage were both negligible, so the whole of this loss was due to dielectric hysteresis.

[illegible] The [illegible] 154 watts.

It cannot be said that 2,226 apparent watts and 275 true watts per mile are negligible quantities. This is at 2,000 v. 100 ∿.

When the cable is worked as intended at 6,000 volts 50 ∿ the losses per mile will be—

·86 × ·6283 × 3 = 1·621 amperes.
The apparent watts per mile 1·621 × 6,000 = 9,726 :
and the true watts 9,726 × ·124 = 1,206 per mile.

This of course is on the assumption that the power-factor is the same at high pressures as at low (as seems probable), and that the loss in this instance was accurately determined.

The dielectric loss is not preventible. It may be lessened by increasing the thickness of the dielectric—that is, by reducing the strain—or by using a dielectric of low specific capacity, and by working at a low periodicity; but for any given case it must be accepted as one of the conditions of working. In some respects it is like the hysteresis loss accompanying the magnetising and demagnetising of iron.

But dielectric hysteresis seems to differ from magnetic hysteresis in showing no saturation effect. Possibly at very high pressures the curve may begin to bend down, but I should expect the insulation to break down earlier than the proportional law.

I have already said that the actual loss in the dielectric, whatever its amount, is going on always when the mains are energised—whether any power is being transmitted or not. In the case just examined of the 6,000-volt 50 ∿ transmission it amounts to about 0·23 watt per foot of cable. So far as it is a cause of waste of power this may be, and often is, a much more important loss than the ordinary C^2R copper loss. This will be seen from the following comparison :—

The 37/15s cable (0·154 sq. in.) has a resistance of 0·271 ohm per mile, or 0·542 per mile for the two conductors. The constant dielectric loss of 1,206 watts per mile is therefore equivalent to a copper loss due to the constant passage through both conductors of 47·2 amperes, or at the current density of 326 amperes per square inch ; or a current that would give a drop of 25·6 volts per mile (or in the singl

[illegible] amperes = 400 amperes per sq. in.), or 0·46 [illegible] on a 2,000-volt circuit. If the cable is [illegible] its dielectric uses up 10,564 B.T. units [illegible]

[illegible] this [illegible] I have worked out tables [illegible] 2,000 [illegible] 500 volts, taking five commercial sizes [illegible] and giving the dielectric losses, and [illegible] be equivalent to those dielectric [illegible] the dielectric power-factor as 0·124 as [illegible] may, of course, vary with different cables.

TABLE II.

[illegible] 2,000 volts, 50 [illegible]

[illegible] PER MILE.

Size of Cable [illegible]	[illegible] 20	[illegible] 20	10/16	10/14	37/16
[illegible]	[illegible]	[illegible]	·000	·004	·118
Resistance [illegible]	[illegible]	[illegible]213	·7	·45	·36
Capacity [illegible]	·33	[illegible]	·6	·7	·75
Apparent Watts [illegible]	[illegible]	[illegible]	754	880	944
True Watts [illegible]	[illegible]	[illegible]	93	109	117
[illegible]	[illegible]	[illegible]	[illegible]55	15·6	18
[illegible]	[illegible]	312	102	165	153
B.T. Units per Mile per Year [illegible]	455	[illegible]	815	955	1,025

TABLE III.

Cables working at 7,500 volts, 50 ∿.

LOSSES IN DIELECTRIC PER MILE.

Size of Cable	17/20	19/20	19/16	19/14	37/16
Mfd. per Mile	·2	·33	·364	·425	·454
Apparent Watts... ...	3,300	5,450	6,000	7,000	7,500
True Watts...	410	670	744	870	930
Current (and current density) in conductor that would give loss equivalent to that in dielectric ... ... — Amp.	8·3	17·4	32·6	44	50·7
— Amp. per sq. in.	1,180	913	540	467	430
B.T. Units per mile per mile	3,590	5,870	6,520	7,630	8,147

NOTE.—In these two tables the equivalent copper loss is taken as that in a single conductor—not in a "lead and return"—for the reason that the dielectric loss is the same (for the same capacity) whether the main is a single one or double.

Take a higher pressure illustration. Imagine a system of 40 miles of cable working at 20,000 volts 50 ∿, the capacity being 0·5 mfd. per mile. With no load and no apparatus connected to the mains the generator would have to provide at charging current of 125·6 amperes at 20,000 volts,

= 2,512,000 apparent watts.

If the power-factor is ·124, then the actual energy absorbed by the mains will be 311,488 watts, and the number of B.T. units consumed per year will be 2,728,633, or an annual output that is exceeded by only a very few electric supply stations in this country. The loss works out at 1·5 watts per foot of the cable with this power-factor.

One rather unfortunate property of cables is shown by these tables. I refer to the fact that as different sizes of cables, made systematically to be suitable for safely working at any given pressure, have capacities which do not lessen very much with decrease of size, the dielectric loss of small cables is therefore disproportionately large. For example, in the five sizes given, the section increases in the ratio 1 to 17, but the capacity only as 1 to 2·28.

The carrying power of a cable is limited in most cases by drop of volts rather than by heating or loss of power

otherwise this dielectric loss would very much restrict the load that could be put on cables.

Nevertheless, this loss seriously affects the question of raising the pressure for long distances or large powers, at least where underground or covered conductors have to be used. As the copper loss is inversely as the square of the current, the temptation is to increase the pressure and reduce the current. But the dielectric loss being proportionate to the square of the pressure, and being moreover a constant or all-day loss, whereas the copper loss is only fully felt at times of full load, limits to increase of pressure are imposed which may be reached sooner than has been supposed.

This opens a field for very careful consideration—and balancing of advantages—of high and low (or lower) pressure. I do not attempt to make any comparisons now. Every case must be examined on its merits and due weight given to load-factor, drop, value of power, plant capacity, and so on.

I believe the general opinion has been that the production of capacity current involved practically no expenditure of energy; that it was in reality a wattless current, and that the only waste—a sufficiently considerable one—was in running the alternator under light load. I think nothing contrary to this opinion was put forward before the recent parliamentary committees on power bills. Those committees were assured, for instance, that so far as power was concerned, the losses with underground cables were the same as with aerial lines. I fear this is by no means the case. I do not wish to adopt an alarmist tone on this subject, but I think it will be admitted that if the tests referred to above are even only approximately correct, the subject deserves the very serious consideration of electrical engineers.

It is probable that those who have had to do with extra high-tension mains, or even with long or extensive mains working at moderate high pressures, will have noticed, as I have, that when switching such mains on, the engine has been checked much more than would be consistent with the production of merely wattless current.

We may now pass from the question of the power-factor the true power, to the wattless part of our "apparent

watts." This we see is about 87·6 per cent. or 7-8ths of the total.

It is very desirable we should do anything we can to lessen the production of "wattless current" by the alternator, as the amperes are real even if the watts are not.

In producing "wattless current" at full pressure the alternator is taking a good deal of power, and is working very wastefully as to steam consumption. It takes practically as much plant to produce the current as if the energy were real. In fact it might easily happen from the known wastefulness of engines working at light load and from other causes that the true energy would cost less to produce it than the false or wattless energy. For example, to charge 20 miles of the 6,000-volt cable referred to above (and assuming that no means are used to counteract the capacity) the alternator would have to be run at 6,000 volts sending out $1{\cdot}621 \times 20 = 32{\cdot}4$ amperes = 194,500 apparent watts or 260 apparent E.H.P. This would require a 200 kw. alternator fully loaded so far as current is concerned, although the true energy would only be $194{\cdot}5 \times {\cdot}124 = 24$ kw.

As there are objections to running a large alternator by a small engine, a large combined plant is often run on a low power-factor circuit.

Fortunately the difficulty can be avoided very simply, by the application of known principles. To some extent this is done now, but I think it is done accidentally or unconsciously, or at least without system.

In explaining how the wattless current may be reduced and the power-factor raised to unity or nearly so, I hope those who prefer the mathematical treatment of such questions will not be impatient at my attempts to express my meaning in the vulgar tongue.

In a conductor or circuit having capacity the charging current has a positive phase displacement of 90° from the E.M.F., while the current in an inductive circuit has a negative phase displacement of 90° from the E.M.F. Thus there is a difference of phase of 180° between these two currents. This is well known. Its systematic application affords a solution of the problem we are now considering.

Imagine an alternator supplying a circuit having capacity. Then if an inductance or choking coil is put in parallel with the capacity, and if it is so designed and adjusted that it takes

a wattless self-induction current equal in amount to the wattless capacity current of the mains the two will balance one another, and the generator will not have to produce any wattless current for either the capacity or the self-induction. Each will take its full current, which will pass to and fro between the capacity and the choking coil, being alternately a capacity or charging current with a positive phase displacement and a self-induction current with a negative phase displacement. The generator will only have to keep up the E.M.F. and to produce the energy component of the current for the cable and the energy component of the current for magnetising the choking coil, and for leakage

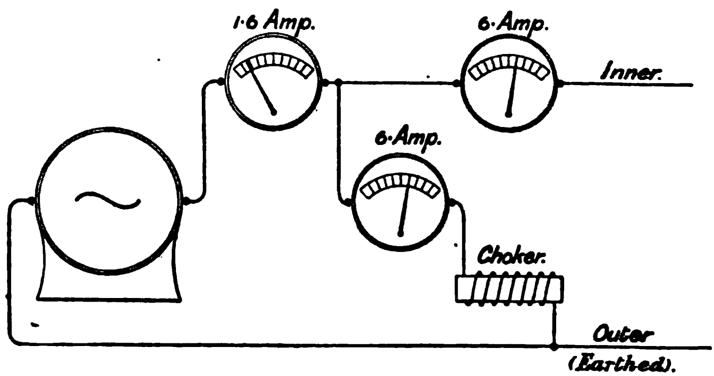


FIG. 4.

and resistance losses. As these energy currents are in phase with the E.M.F., the alternator will act as if working on a circuit having neither capacity nor self-induction—that is, a circuit having a power-factor of unity. This will at least be the case if the balance is perfect.

By the kindness of Mr. Sparks and the County Company, I have been able to try some experiments on the $5\frac{1}{2}$ miles of mains already alluded to. The arrangement is shown in Fig. 4.

I made a choking coil to take 6 amperes at 2,000 volts 100 $\sim$. Particulars of this coil are given later on. When working at 2,000 volts 100 $\sim$, the alternator gave 6 amperes when connected to the cable alone. The current was also 6 amperes when the alternator was working on the inductance coil alone. When the cable and the coil were

put in parallel, with ammeters in circuit as shown, the alternator produced 1·625 amperes only, although there was still a current of 6 amperes in the cable and 6 amperes in the choking coil.

There was not a perfect balance. The true watts taken by the cable and the choker were about 2,000, therefore the alternator current should have been only about 1 ampere, but I was not able to get a closer adjustment. I must have had very nearly the best balance obtainable, as any adjustment of the choking coil, either in the direction of increasing or of decreasing its current, caused an increase in the alternator current. Possibly the slightly imperfect balance was due to a difference of effect of a sine curve alternator on the choker and on the capacity.[1] The actual practical result, however, was quite satisfactory, as the choking coil effected a saving on the system of about 9,000 apparent watts, the energy absorbed by it being about 500 true watts. I need not say that the saving under these circumstances is not only "apparent."

It is not easy to say what would be a fair estimate of the cost of producing "wattless energy" I venture to think it is not much less than one-fourth of the cost of true energy.

Whatever figure is taken, the desirability of economising in such matters will be realised when I point out that at one penny per E.H.P. – hour one watt for one year costs one shilling,[2] or say £1 capital.

DESCRIPTION OF CHOKING COIL.

This description is not given as that of a very satisfactory example of design, but to show the coil used, and in the hope that it may be of some interest to designers of such apparatus. The main dimensions of the coil used in these tests are given in Fig. 5. It consists of a set of E stampings, with a coil wound about the middle projection. The "yoke" is straight and is carried on supports, allowing of adjustment of the gap. Fig. 6 gives the currents with various air-gaps.

The current taken by the coil at 2,000 volts 100 ∿ with various air-gaps is given by Fig. 3. The form was chosen in preference to the simple straight coil form, as a convenient one for adjustment, and because it gives an external field more suitable for enclosing in an iron core.

[1] See paper in Journal XXIX., p. 154, Jan., 1900, by Alexander Russell, according to whom a sine curve wave gives a smaller condenser current and a larger magnetising current than any of the other wave shapes considered in his paper.

[2] Or, more precisely, 11·74 pence.

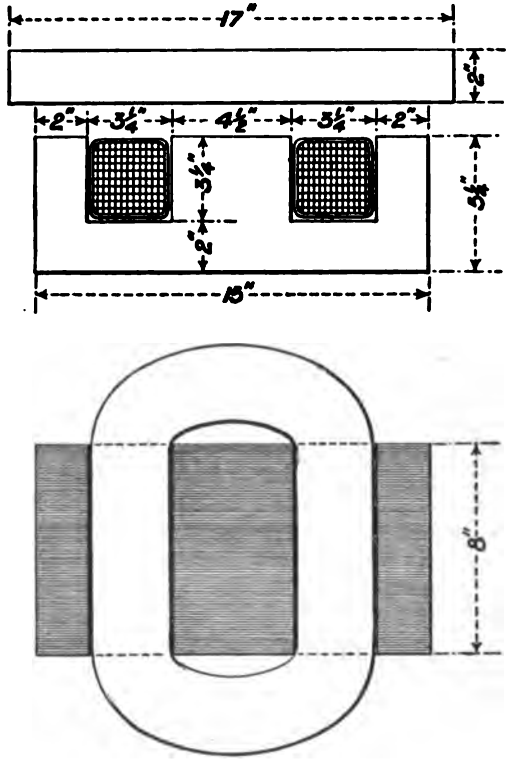


Fig. 5.—Choker for 12 Apparent K.W.

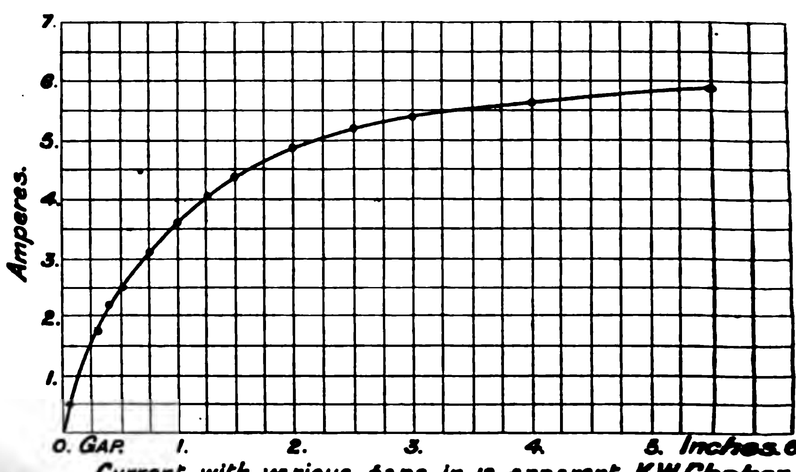


Fig. 6.

The cross-section of the core is about 29·76 sq. in., or 192 cm. of actual iron—weight about 190 lbs.

The winding consists of 756 turns of 0·08 in. wire, having a total length of about 2,600 ft. and a resistance of about 4·1 ohms.

At 2,050 volts 100 ∿ , N = 6,081,000, B = 3,170.

The iron is 0·014 in. thick of a quality which would have a loss in a closed-circuit transformer of about 0·55 watts per pound. The iron loss should, therefore, be about $190 \times 0·55 = 104·5$ watts, with the yoke closed down. With the yoke removed (as used) it should have a loss of about 68 watts if the circuit completed itself through the air.

The copper loss with 6 amperes $= 6^2 \times 4·1 = 148$ watts. Total, 216 watts.

The tests, however, show a loss of about 500 watts. The difference must be due to eddies caused by magnetic leakage. The stray power is higher than would be expected, but it answers its purpose very well. Its power-factor is $\frac{500}{12,500} = 0·041$.

I know of no published information on the losses in choking coils, and suggest it as a useful subject for study. The principal difficulty is in testing apparatus having such low power-factors.

The weight is about 22 lbs. per apparent K.W.

I have said that in reducing the capacity current required to be produced by the generator, something was effected in ordinary practice, but accidentally, or at least not systematically, by the action of all self-induction in the system—for example, that of transformers or motors. In transformers the effective self-induction is very small. The no-load current is small in good closed-circuit transformers, and the power-factor is high—usually from about 0·7 to about 0·85. The wattless component available is therefore very small—at least, in modern systems having large and efficient transformers. No doubt if the conditions of a circuit are known from the first, the transformers may be so designed as to provide the capacity current, at least at light load; but it would be difficult to do this systematically as the conditions in practice are so variable. With a large number of small transformers it may be that the wattless capacity current may be compensated or more than compensated.

I do not think transformers of low power-factor should be used. I believe the better plan will be to continue to make transformers with small no-load current and high power-factor, and provide separately for any compensation that may be necessary.

When motors are used, their large, idle, lagging current is available to compensate the capacity. Where there is a large motor load the idle current will usually be a lagging rather than a leading current, even on mains of considerable capacity. A synchronous motor, as is well known, acts either as a capacity or a self-induction according to the excitation. When over-excited, it acts as a capacity and takes a leading current from the mains; when under-excited, it acts as a self-induction, taking a lagging current. When excited to take a minimum current it has no idle component —that is to say, its power-factor is unity. Such motors running light are now often used for balancing self-induction, but I have never seen them used for balancing capacity. In any case they are a rather expensive remedy, as they have to be practically as large as if they were to do real instead of only apparent work. Their first cost, their working costs, and losses are therefore high. For balancing self-induction they may be necessary. I hope, however, that for this purpose condensers will again be taken up by Mr. Swinburne, or somebody else. Probably their hysteresis loss will not be as great as the losses in motor compensators nor their working cost or first cost as high.

But in any case, the running of under-excited synchronous motors is an expensive and clumsy way of compensating capacity, compared with simple choking coils, such as I suggest.

Such coils, used with a phase-indicator, should be useful in all systems where the capacity current is large enough to deserve attention.[1]

There is something very disproportionate and anomalous about the two classes of loss in the dielectric—the insignificant losses due to leakage, and the large apparent and actual losses due to capacity and hysteresis.

Let me take for illustration a 10,000-volt cable having one-third microfarad capacity per mile, and working at

[1] I may mention that when I applied for a patent for this method my attention was drawn by that useful institution, the German Patent Office, to the fact that, so far as concerned the treatment of a long transmission line by choking coils placed at intervals, I was anticipated by Charles Schenck Bradley, the well-known American electrical engineer (British Patent 20,493 of 1897). Reference should also be made to the proposals of our president of last year, Prof. S. P. Thompson, to compensate the capacity of submarine and other cables to facilitate signalling by placing high-resistance choking coils at frequent intervals along their length. See his British patents 22,304 of 1891; 13,064 and 15,217 of 1893; and 13,581 of 1894.

50 ∿. Its insulation is, say, 2,500 megohms (it may be only a few megohms without affecting the argument). The dielectric has several functions. It acts as a nearly perfect non-conductor. But it does conduct a little: it allows $\frac{1}{250,000}$ ampere to pass through it, $=$ 0·04 watt per mile.

Then there is the electrostatic action. From Table I. we see the capacity current is 1·05 ampere, the apparent watts 10,500, and the real watts 1,296, or 32,400 times the watts lost by conduction.

This is an interesting state of things. Both dielectric conduction and dielectric hysteresis waste energy in the same way—by heating the dielectric. We use an insulator which reduces the leakage energy loss to ·04 watt per mile, but allows another energy loss 32,400 times as great.

So far as loss of energy is concerned we should be just as well off—or as badly—with an insulation resistance of $\frac{10,000}{1{\cdot}05} = 9,524$ ohms, instead of 2,500,000,000. The energy spent on our dielectric would be just the same, and I suppose we should have no wattless current at all! I point this out to illustrate an anomaly, not to make a practical suggestion. It may be that no low resistance insulator can be found capable of resisting breakdown under such conditions.

I have spoken of the energy component of the capacity current spent on heating as if it were in some way different from the leakage current. It only differs from it in amount. It is not recovered like the true charging current. It does not surge to and fro in the cable. In all practical essentials it is a leakage current. It goes right through the dielectric, and heats it in its passage exactly like the leakage current. The effect of capacity in allowing the passage of this current is precisely as if it reduced the insulation resistance in the proportion I have suggested, viz., from 2,500 megohms to 9,524 ohms.

At least this is how it appears to me.

With aerial lines capacity effects are comparatively unimportant. For example, with wires of ⅓-inch diameter, hung two feet apart, the capacity per mile is only 0·018 microfarad. Not only is the wattless current therefore small, but there is no "wattful" current.

An Alternate-current Wattmeter.

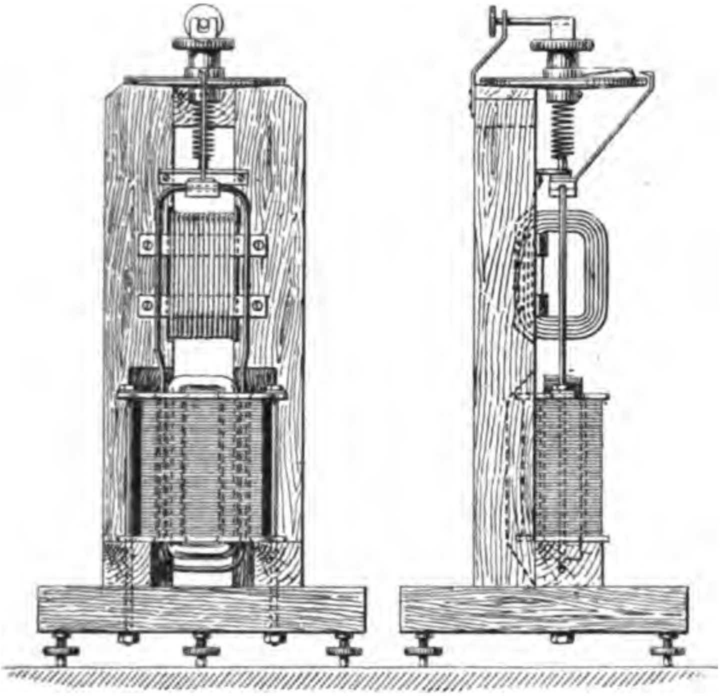
In concluding this paper I wish to describe an alternate-current wattmeter especially suitable for measurements such as are required in connection with the subjects treated in this paper. In devising this instrument my aim was to get a simple, strong, reliable wattmeter—one that needed no resistance added in its E.M.F. circuit, and that would absorb very little energy in that circuit even for large E.M.F.'s. In such an instrument the E.M.F. coil should have a large force, and at the same time be practically without self-induction, and should be correct with all power-factors met with in practice. If possible it should be correct through a considerable range of periodicity.

Long and satisfactory experience of the Siemens dynamometer under workshop conditions made me turn to it as the most suitable type of instrument, and to try to devise means by which it should have the qualities just enumerated.

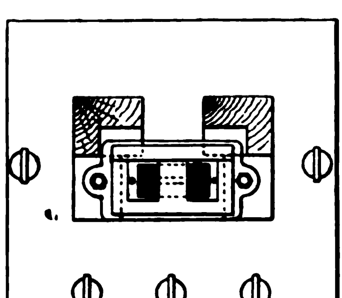
Fig. 7 shows the instrument in elevation and plan. Fig. 8 is from a photograph of an actual instrument.

The Siemens dynamometer form is closely followed. The series or current coil is placed in the ordinary position. Under this coil is a small transformer, the primary winding of which is wound for any required E.M.F. Next to this primary winding a space is left at each side—the space usually occupied in a transformer by the secondary winding. In this space a closed coil of one turn of wire is suspended freely—the wire is platinoid, manganin, or some other conductor having a negligible temperature co-efficient. This conductor is suspended and controlled exactly like the suspended coil of a Siemens dynamometer. It acts as the secondary of the transformer. As it is closed on itself no mercury cups are needed. Its circuit external to the transformer consists simply of a loop passing up the front and down the back of the fixed or current coil.

The action will now be understood. A comparatively large current of very low pressure is generated in the secondary coil. This current is in opposite phase to the E.M.F. It passes through the field of the fixed or current coil. The suspended conductor is acted on and deflected by the current in that coil, and is brought back to zero by the tension of the spring as in an ordinary dynamometer.



FRONT ELEVATION. SIDE ELEVATION.



PLAN AND SECTION THROUGH TRANSFORMER.

FIG. 7.

It will be seen that so long as the iron of the transformer is not forced to too high a density by using too great an E.M.F. or too low a periodicity, the secondary current will be proportional to the E.M.F. and independent of the periodicity. The magnetising component of the primary current and the energy spent in the iron will vary with the E.M.F. and periodicity, but if the self-induction of the secondary coil is sufficiently low this will not affect the proportionality of the secondary current to the primary E.M.F.

The instrument shown on the table is the first I have had made. It has been kindly constructed to my drawings by Messrs. Siemens. By tests made at the Board of Trade Electrical Laboratory (in connection with which I have to thank Mr. Trotter and Mr. Rennie), I find my anticipations have been verified—the instrument has the same constant with power-factors of 1 and of 0·1, and it is proportional all round the scale.

This kind of instrument may easily be made for a large range both of E.M.F. and current, as the primary of the transformer as well as the current coils may be variously wound or connected—the suspended secondary conductor being always the same.

In the instrument exhibited the current coils are three in number, of varying section—the transformer primary being suitable for any E.M.F. up to 500 volts at 100 ∿. The range is as follows:

Fine coil,	for maximum of about	2·5	amp., constant	1·25	watts per division.
Medium	„ „ „	15	„ „	6·8	„ „ „
Thick	„ „ „	90	„ „	37	„ „ „

This is at 83 ∿. There are 400 divisions in the circle.

I have not succeeded, in this first attempt, in making the instrument independent of periodicity. The variation is about 10 per cent. between 30 ∿ and 90 ∿. In using the instrument it is therefore necessary to know the periodicity. I hope to greatly reduce this variation, or perhaps get rid of it altogether through a sufficient range for practical purposes. Even as it is, it will perhaps not be considered a very serious objection.

The principle of this instrument may be readily applied

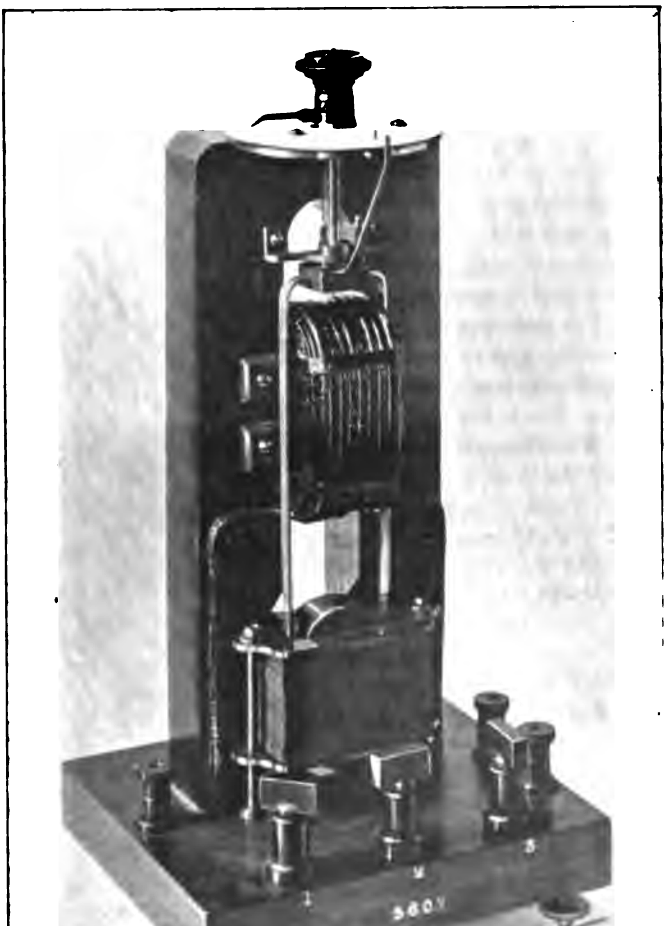


Fig. 8.

to other forms, but it would be premature to describe them here.[1]

In this paper I have not attempted to treat of several aspects of "Capacity in Alternate Current Working"—matters perhaps as important as those I have tried to make clear. I have avoided comparisons between different kinds of cables. I have considered single-phase only, not two- or three-phase. I have said nothing of effects of capacity on regulation, on distribution of potential along lines, on liability to rupture of insulation due to capacity, or on means for reducing such liability. In keeping to one aspect of the subject, I have hoped to be of most use to my fellow Electrical Engineers.

I have to express my thanks, for information or facilities afforded me, to Mr. Gray and the Silvertown Company, to Mr. Edmunds and Mr. Howard of Messrs. Glover & Co., to Mr. Nisbett and the British Insulated Wire Company, and to Mr. Gavey (Chief Engineer of the Post Office). Also to Mr. Dallas and Mr. Grafton, respectively of the Silvertown and the County Companies, for assistance in experiments. I have already mentioned my indebtedness to Mr. Sparks and the County of London and Brush Provincial Company.

Professor W. E. AYRTON: The first two or three pages of Mr. Mordey's paper, he tells us, are given in order to impress upon central station engineers that they have in their possession a very easy means of measuring the capacities of their cables, and I presume by that he implies that they do not use these means. If that be the case, then who is to blame? Is it because the method has not been put before them? or is it because, after the method has been put before them, they have neglected it? The matter of measuring the capacity of real cables in this way was brought prominently before me a good many years ago. It was brought to my notice in the ~~very~~ beginning of the Deptford Generating Station when, as you know, power began to be transmitted from Deptford to the Grosvenor Gallery, through six underground cables each some six miles in length. I was very often at that time in Deptford with Mr. Ferranti, and one day Mr. Ferranti put to me this question: he said, "Can you explain this to me, because to me" (that is, to Mr. Ferranti) "it is very curious. I find that during the daytime, when there is practically no load on in the Grosvenor Gallery, the ammeter reads a fairly large value, and when the load comes on in the evening, the ammeter reading does not go up very much. What does it mean?" **Professor Ayrton.**

[1] I should mention that some years ago Mr. Swinburne proposed or made an instrument in which he pivoted a closed-circuit conductor—this had a current induced in it, and was deflected by the flux in an air-gap in which it hung.

[illegible]

[illegible] Mr. Mordey is irrelevant. [illegible] alternate currents; in fact, [illegible] direct currents are [illegible] It would therefore have [illegible] deals wholly and solely with [illegible] alternate current method of measuring [illegible]

[illegible] referred, that was given in 1891 before [illegible] called "Interference with Alternate Currents," [illegible] that name is because it dealt not only with [illegible] referring to, but with the next part of Mr. Mordey's [illegible] the combination of self-induction with a capacity. [illegible] that were actually obtained in this way, and [illegible] for March 13, 1891, it is stated in the [illegible]

[illegible] inductive coil in parallel, an ammeter [illegible] amperes, whilst those in the branches [illegible] through the condenser and 10 amperes [illegible] nature were described [illegible] the two parts [illegible]

mentioned." It is interesting to notice that, as a matter of fact, this ratio was exactly the best result that Mr. Mordey could obtain with his combination described in his paper of to-night, *i.e.*, ten years later. Professor Ayrton.

The abstract in the *Electrician* goes on to say: "Theoretically this ratio might be anything, depending as it does on the phases of the pressures in the two parts, and these phases are determined by the ratio of the impedance of the coil to its resistance; practically, however, it was not easy to get a coil of large self-induction and very small resistance."

This last remark is interesting in showing that Dr. Sumpner and I were occupied at that time *not* in applying a condenser to a self-induction, but in carrying out exactly the experiment described by Mr. Mordey in his paper of to-night, viz., that of applying a self-induction to a capacity for the purpose of reducing the capacity-current that would otherwise flow in the leads bringing the current to the condenser.

But now, if we take Mr. Mordey's own experiment, I say that by dealing with the principles given in that 1891 paper of ours, without going a step further—and that is the best proof of what was in that paper—I can enormously improve his result.

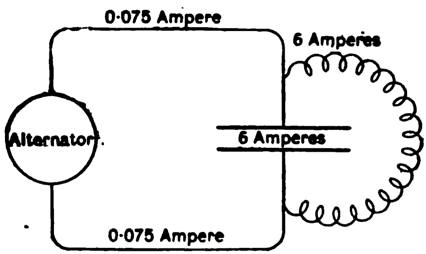


FIG. A.

For if F be the capacity of a condenser, or cable, in farads, p be 2π times the frequency of an alternator sending current into the cable, V be the R.M.S. value of the sine wave of P.D. produced by an alternator, and o the resistance, in ohms of an inductive resistance placed as a shunt to the condenser, then the current in the alternator circuit will be a minimum and equal to

$$\frac{\mathrm{VF}o}{p},$$

when h, the self-induction of the inductive shunt, has a value

$$\frac{1 + \sqrt{1 + 4\,\mathrm{F}^2 p^2 o^2}}{2\,\mathrm{F}p^2} \text{ henry.}$$

Now, in Mr. Mordey's case this best value of h becomes 0·53 henry, and with a shunt to his cable, *made without any iron* and having this self-induction and a resistance of four ohms (the resistance of his choker), the alternator current will be found to reduce itself to as little as 0·075 ampere, if V be 2,000 volts, the frequency 100 ∿, and the current due to dielectric hysteresis is inappreciable.

Professor Ayrton.

Mr. Mordey ought to have used no iron at all in his choker, and what I propose to do is to supply Mr. Mordey with an inductive coil properly constructed for his case, and he will find that, with sine waves, the alternator-current will be much less than 1·6 amperes, and the power wasted in my coil much less than the 500 watts which was expended in his choker.

Mr. Mordey will say, But how is it possible with any coil of any shape whatever to reduce the current to anything like that when you have this dielectric hysteresis loss? That brings me to really the most important part of his paper, that is to say, the loss in the cable. I should very much like to ask Mr. Mordey whether he has sufficiently tested either the cable or what he calls the "Thomson recording wattmeter"—for him to feel at all sure that, with the low power-factor he is dealing with, the instrument indicated correctly. Mr. Mordey very rightly said that he did not use an ordinary wattmeter, because with such a low power-factor it was very difficult to get accurate results; but he seems to forget that the error in the Thomson watt-hour meter, as it ought to be called, or energy meter, is even much larger. If he will refer to a paper in the *Electrical Engineer* of March 24, 1893, he will see the question of the Elihu Thomson's watt-hour meter dealt with. The error in the watt-hour meter—and how it can be calculated—is discussed; and then experiments are given showing that the actual errors when the instrument is employed on an inductive circuit is as is given by the formula quoted. Now, if you will take that formula—which is the formula I worked out years ago, for a wattmeter—and apply it for such a low power-factor, you will find, unless I am very much mistaken, that the power indicated by an energy meter may be considerably larger than the true power.

I feel certain, therefore, that the explanation of Mr. Mordey's high power-factor is either that his energy meter—for the reasons stated in this paper in the *Electrical Engineer* of March, 1893—gave an answer which is much higher than the truth, or the cable that Mr. Mordey used was an exceptionably bad cable.

The matter is of enormous importance. We are not dealing with any question of priority, or whether central station engineers have or have not studied what has been written; but we have to consider this—What is the true loss in a cable? I mean the loss due to this so-called dielectric hysteresis. One reason why I am inclined to think that it is far less than Mr. Mordey imagines is that in all previous measurements which have been made by myself, by Major Cardew, and others, the power-factor has come out far smaller than the value Mr. Mordey gives now, the power-factor meaning the ratio of the true power wasted in dielectric hysteresis to the apparent power given to the cable. In the case of Mr. Swinburne's own condensers we found that it was something of the order 0·01. In the experiments made by Major Cardew which appeared in the Institution's Journal in 1893, the values vary in different experiments from about 0 watts loss to 4·8 watts loss. If you take his highest value, his power-factor is 0·063; if you take his lowest value, it is nought; and taking the mean we see that he was dealing with a power-factor under 0·03.

If, again, you refer to experiments which have been made by Lombardi on this very subject of the hysteresis loss of various dielectrics, you will find that viscous substances like paraffin wax appear to have a power-factor of about 0·088, and gutta percha 0·042. Lombardi's results are very interesting, because for petroleum he got 0·014, and for condensers which have petroleum smeared on them or poured on them we found years ago, in 1894, I think, the power-factor was about 0·01. The result seems to be this: that, if the cable which Mr. Mordey tested has really a power-factor of 0·124—that is to say, if one-eighth of the whole of the power given to the cable is really wasted—then the cable is made most unnecessarily bad, because if you make the cable with any of the other materials referred to above, you ought to get a power-factor very much smaller. It is a matter obviously that can be very carefully tested, and it is a matter which I thoroughly agree with him ought to be tested if there is really any doubt about the matter, because it is of vital importance in connection with these power transmissions through long lengths of underground cables. In order to test it, I would ask Mr. Mordey if he would be good enough to allow me to find out the exact nature of the error—whether the instrument used was an ordinary wattmeter or an integrating wattmeter — which led it to give this extraordinarily high power-factor. Professor Ayrton.

The PRESIDENT: Mr. Mordey asks me to allow him to make some reply to Professor Ayrton's remarks, without waiting until the next meeting. The President.

Mr. W. MORDEY: I wish to say before the meeting closes that I have every reason to believe the power-factor was properly determined. The wattmeter was calibrated in the well-equipped laboratory of the Westinghouse Company, especially with a view to finding out its constant with low power-factors. I accepted the readings of the instrument. If I had not felt that the fact of a considerable loss of power in mains was confirmed indirectly and broadly by general observations in central stations, I should not have said anything about the power-factor at all. I have not any doubt whatever that there is a considerable loss of power in the insulating materials of cables. If this is the case, you will agree with me that it is sufficiently important to merit attention. I do not want you to go away and think that I have put forward a figure of that sort, a figure that is very important indeed in practical engineering works, without having a reasonable belief that it was based on accurate facts. However, the instrument can easily be calibrated or further tests taken, and I will, before the end of the discussion, see if I can give further information. Mr. Mordey.

On the motion of the PRESIDENT, a hearty vote of thanks was passed to Mr. Mordey for his paper.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Members:

Sir John Wolfe Barry, K.C.B., F.R.S.

Bertram Sands Giles.

Sidney Howe Short.

Associate Members:

Alan Neville Banister.
John W. Black.
Watson Blagburn Carrick.
George Alexander Clark.
James Herbert Clothier.
John Wylie Donaldson.
Christopher Elliott.
Alexander Gerrard.
Arthur Hewitt Gilling.
A. J. Harris.
John McNab Hunter.
William Livingston King.
Edgar John Kipps.
William McAulay.
Peter Alexander McKillop.
William Nairn.
Walter Reilly.
Thomas Roles.
John Francis Stanley Scott.
John Severs.
Thomas William Sheffield.
Christopher J. Spencer.
James Ross Stevenson.
Charles Granville Vines.

Associates:

Harry Allcock.
Alan Arthur.
Joseph Bein.
George C. Blair.
Alec David Chalmers.
Ardesir Bomanjee Darookhanawalla.
Edwin Henry Dixon.
Paul Goldschmidt.
Edmund Goolding.
Thomas Stephen Hepworth.
William Hodgkinson.
James Lowson.
William Maclay.
William Mayer.
Edwin Morgan.
Henry James Moysey.
Gerald Mushet.
Felix Bernard O'Hanlon.
Franke Herbert Parker.
Frederick George Thomas Parsons.
William Richards.
Frederick William Richardson.
Alfred James Ryan.
Valentine Aloysius Ryan.
Edward Ashmore Thompson.
George Hamilton Thomson.
Fiennes Olive Trotman.
Charles Alfred West.

Students:

John Cruickshank Anderson.
John Alexander Armstrong.
Raymond Dorrington Bangay.
Arthur Wynne Barnley.
Frank Norton Bell.
Charies Henry Day.
Hugh William Geare.
Alexander Walter Harrold.
Horace William Holt.
John William Law.
William Marden.
William Robinson Myers.
Lionel George Nunes.
Frank Oldrieve.
Hubert Beaumont Shepheard.
Alfred Symth.
Vincent Dare Sorby.
Henry D. Stepanian.
Claude Stert.
Joseph P. Tierney.
Werner Anton Trier.
Frederick Wardrobe.
Hildred Edward Webb Bowen.
Richard Hubbard Welch.
Cecil Tom Wilkinson.
John Michael Faraday Wilson.
Horatio Peter Stanley Wise.
Joseph Julius Wolff.

The Three Hundred and Fifty-seventh Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, 25, Great George Street, Westminster, on Thursday evening, February 14th, 1901, —Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on January 10th, 1901, were read and approved.

The PRESIDENT: I took it upon myself to postpone the last Ordinary General Meeting which had been announced for January 24th. It was, I think, on Tuesday night that I felt that Queen Victoria being then very seriously ill it would be right to postpone the meeting. There was no time to call a Council meeting nor to send out postcards, but notices were sent to the daily papers and to those electrical journals that were likely to be published in time. At a Special Meeting of the Council of the Institution of Electrical Engineers held on the 7th February, the following resolution was carried unanimously:—

"That the Council of the Institution of Electrical Engineers, in Special Meeting assembled, hereby records its deep sense of the irreparable loss which the British Empire has sustained through the lamentable death of Her Majesty Queen Victoria, and its sorrow, in which each member shares, that one, who spent Her life for the good of Her people, and to whom Her subjects were affectionately devoted, has been removed from the scene of Her unremitting labours, and that a reign marked by unparalleled social, scientific, and industrial progress has thus been brought to a close.

"The Council humbly begs permission to express to His Majesty King Edward, and to the members of the Royal Family, its most sincere condolence and sympathy, and further to lay before His Majesty the assurance of its unswerving loyalty and devotion, and its earnest wishes that He may, with Her Majesty Queen Alexandra, long be spared to reign in happiness and peace over a loving and united people.

"It therefore directs that a sealed copy of this Resolution, with copies of Resolutions of Condolence passed by Local Sections of the Institution, be forwarded to the Home Secretary for transmission to His Majesty."

I may say that copies of resolutions have been received

[illegible]

[illegible]

[illegible]

[illegible]

[illegible] Electrical Engineers [illegible] present [illegible] the hearts [illegible]

[illegible]

"[illegible] the Committee [illegible] behalf of the [illegible] Section [illegible] the Institution of Electrical Engineers [illegible] by the death of [illegible] Sovereign Queen Victoria, whose long and beneficent [illegible] social and scientific progress."

"[illegible] of condolence and loyalty be sent to His [illegible] with the hope that He may long [illegible]."

[illegible]

"[illegible] the Manchester Section of the Institution of Electrical Engineers, [illegible] the deep grief with which we have received the news of the death of our late beloved Sovereign Queen Victoria."

I have now to move that the Council Resolution be adopted by the Institution.

The resolution was then carried unanimously, the members rising in their places in silence.

The names of new candidates for election into the Institution were announced, and it was ordered that they should be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associate Members to that of Members—

John Reid Dick.
James T. Rossiter.

From the class of Associates to that of Members—

Frank Broadbent.
Alfred Cecil Eborall.
Henry Herbert Reynolds.

From the class of Associates to that of Associate Members—

Trevor Duesbury.
William Jones.
C. McCarthy-Jones.
Percy Sewell Sheardown.
Sydney Cummins Smith.
William Paul Steinthal.
Edwin A. Uttley.

From the class of Students to that of Associates—

Arthur Daniel Crowther.
Charles Jones Lockyer.
C. Macmillan.
Christopher Oscar Milton.
Charles N. Nettley.
Kenneth Charles H. Newman.
Arthur Woolmore Wigram.
Herbert T. Wilkinson.
Harold Henderson Williams.
Walter Trevelyan Wright.

Messrs. J. M. Elliott and E. J. Howard were appointed scrutineers of the ballot for the election of new members.

Donations to the Library and Building and Benevolent Funds were announced as having been received since the last meeting :—To the *Library* from Messrs. Blackie and Sons, Messrs. Carré and Naud, the Radcliffe Library, Sir C. Todd (Member), and Mr. A. J. S. Adams (Associate). To the *Building Fund* from Messrs. R. J. Fuller, H. R. Carson, H. Lewenz, S. C. Smith, H. M. Kollé, J. H. Rosenthal, C. F. Farlow, S. L. Brunton, F. Langley, H. G. Andrews, J. L. Vogel, Lionel Wood, H. Kilgour, J. R. Penning, Mark Robinson, Kenelm Edgecumbe, Henry Lea, Colonel E. D. Malcolm, Lord Kelvin, A. G. Hansard, H. G. Thomson, A. Stroh, J. Maclean, Norman Endacott, Robert Hammond, H. J. Garnett, J. R. Bedford, R. O. Ritchie, H. C. Chamen, M. M. Gillespie, A. P. McDouall, A. D. Constable, F. H. Goodall, W. McGeoch, H. G. Beeton ;

... there was nothing ... teaching his pupils for ... discuss.

... be allowed to make a few ... make a correction or an addition to ... of my paper I drew attention ... well-known fact, that from the volts and ... calculate the capacity. I advised central- ... ammeter to take the charging current, and ... that way. *Professor* Ayrton thought I had ... *credit for having* known that fact a great many ... that he claims that as his method, invented ... and taught to his pupils ever since. As a matter ... method that was common property before the date ... no right whatever to claim it. I am, however ... that he has been teaching it ever since. But, since ... the ... meeting in that way, I was led to look ... it might not be right, and I found that ... for the ordinary commercial ... charging current, the ...

practice, and apparently even slight departures from the sine form cause considerable difference in the current. There was not one word of warning given us by Professor Ayrton last week as to the enormous correction that might have to be applied in the use of this formula that he informed us he had been using for so many years. I know the correction is involved in the mathematical expression, but it is not sufficient to use a formula to entitle you to claim that you know all that is involved in that formula. This introduces a new definition of power-factor. If the current varies two or three times for a given voltage, the power-factor may vary in the same proportion, although the energy may be the same. It is very important that I should point that out. I ask those who may do me the honour of taking part in this discussion, if they refer to any results they have obtained, to state whether the power-factor they obtained is based on the ratio of the true watts to the volt-amperes, or is based on the calculated apparent watts with a sine function machine. Mr. Mordey.

There are other points that I should like to refer to, particularly as I gave a pledge at the end of the last meeting that I would try to give some further information on the point as it was left then, but as you, sir, evidently prefer that I should not do it now, I can only ask to be allowed to do it at a later time. I think perhaps it might put the matter on a little more satisfactory basis if I referred to it now.

Mr. C. P. SPARKS: Mr. Mordey in a commencing paragraph of his paper refers to the capacity of cables as a serious drawback to alternating current working. It is true that it is a disadvantage at times of very light load, but this can be largely neutralised by energising a minimum number of cables. Mr. Sparks.

In most E.H.P. stations there are two or more feeders leading to every distributing station, one of which can be used at times of light load, and extra mains can be connected with the rising load, thus reducing any inconvenience due to the leading capacity current to one-half or one-third, as the case may be. As most of the apparatus connected to the system, with the exception of the incandescent lamps, is of an inductive character, the capacity of the cables becomes a positive advantage and improves the load factor. The capacity and dielectric hysteresis of the cable vary with the character of the dielectric and the form of wave curve given by the alternator. The instance given by Mr. Mordey is for a rubber dielectric in which the capacity and power-factor are abnormally high. Had a paper dielectric been used in place of rubber, any inconvenience from capacity would have decreased to one-third for every mile of cable in circuit. Since the date when these cables were set to work, "paper" has almost entirely replaced rubber as a dielectric for such purposes. This is not only due to its lower capacity, but also to its very much lower price and greater durability.

In a later paragraph of his paper, Mr. Mordey alludes to the low efficiency of a large alternator running to supply the capacity current; or rather the large steam consumption of such a set on light load. Up to now, with the limited number of E.H.P. stations in England, the length of cables has in no case been such as to make the capacity current

[illegible] greater economy [illegible] smaller units at times [illegible] sudden demand [illegible] some reserve [illegible] plant. Under these circumstances the [illegible] existing ration [illegible] the capacity current [illegible] means of chokers at times [illegible] as in the [illegible]

[illegible] some explanation [illegible] figures on which Mr. [illegible] paper, namely, those relating to the [illegible] concentric rubber cable. [illegible] since Mr Mordey wrote asking me to give him [illegible] one of our cables, his method of balancing the [illegible] choking coils, and offered to construct a choking [illegible] reduction. Since this date, as pointed out in his [illegible] method was patented by Mr. C. S. Bradley in [illegible] made in March reduced the current from 6 to 1·6 [illegible] connections being as shown on page 577 of Mr. Mordey's paper.

[illegible] these tests Mr. Mordey wrote asking me to consider the [illegible] method to our system of supply, and offered to [illegible] choking coil which would still further reduce the [illegible]. Before coming to any decision in the matter, we made [illegible] which Mr. Mordey has given in his paper, and I may [illegible] these tests were made for our own purpose and not [illegible] of their being used for any publication.

The figures were as follows:—

Cable, Concentric Rubber 5¾ miles; capacity, inner to outer ·86 m.f.; outer earthed; cable disconnected from switch board; insulation 6,000 meg. per mile.

Periodicity	100
Pressure	2,000 volts.
Watts in cable alone	1,509
Watts in choker alone	530
Watts in choker alone with keeper on	495

I may here point out that the meter revolved in one direction when connected to the cable and in the reverse direction when connected to the choker. The watts were then taken with both choker and cable in circuit, the choker having the keeper removed. The energy shown was [illegible], or within twenty-four watts of the tests made separately with leading and lagging currents. Assuming a low power-factor in the first test, we must have had a much higher factor in the later, as the apparent watts were reduced to 3,200 as against 12,000. These figures were forwarded to Mr. Mordey, and he accepted the figure of 530 watts taken in his choker, although he points out in his paper that the calculated loss was 516, and accounts for the difference by eddies.

With regard to the use of Schallenberg Energy Meters for the purpose of measuring energy, a similar instrument to the one used with [illegible] has been subjected to the following tests, the standard

of comparison being a Kelvin Electrometer Wattmeter specially arranged for alternating current-power testing under the supervision of the late Dr. John Hopkinson. In testing instruments against this standard it is found that a particular instrument can be adjusted to read true watts at a periodicity of 100 working on a non-inductive circuit, the pressure being supplied by an alternator giving a sine curve. The same instrument records energy within one per cent. when connected on a non-inductive circuit of fifty periods; thus, under these conditions, there is no periodicity error. On connecting the pressure coil of the instrument to an independent circuit, displaced 90 per cent. cos ϕ thus equalling nothing, the instrument no longer revolves although the full-load current is passing through the series coil. The same instrument calibrated with cos $\phi = \cdot 1$ $\cdot 2$ and $\cdot 3$ records the true watts within 3 per cent., at these phase differences the instrument giving identical readings whether supplied with a leading or a lagging current, (the direction of rotation reversing each time). These instruments have been used to check transformer core losses, and have been found to agree with the figures observed by other methods of measurement (power-factor here ·7 to ·8). Mr. Sparks.

I was not able to be present at the last meeting of the Society, when Mr. Mordey's paper was read, but understand that Professor Ayrton severely criticised the figures given by Mr. Mordey. I, therefore, took an opportunity of reading Professor Ayrton's remarks, from shorthand writer's notes, from which I understand that he took exception to the loss of energy given, and predicted that, if tests were carefully made, the loss of energy would be found to be about one-tenth of the figure given. Under these circumstances, I thought it advisable to supplement the figures previously obtained by some other system of measurement, having in view that a possible error might have arisen in the energy meter used through the fact of it having been standardised on a machine giving a sine curve, and also in view of the fact that the same instrument had not been calibrated at two different periodicities at a low power-factor, although separate determinations were correct.

The method chosen was measuring the difference of power required to drive a generator driven by a direct-current motor. This set consisted as follows:—

(*a*) A two-phase 50-period machine having a capacity of 50 k.w. per phase.

(*b*) A single phase 100-period machine having a capacity of 100 k.w.

(*c*) The direct-current machine generally used for exciting the two alternators, and alternatively for starting them. The machine was constructed for carrying a large over-load for a short period for this purpose. The capacity of the direct-current machine was 60 volts by 125 amperes by 7·5 k.w.

On commencing this system of measurement, the first difficulty met was finding the current largely in excess of that given by the well-known formulæ—

$$C = \frac{V \times \sim \times K \times 2\pi}{10^6}$$

[illegible] Mr. Mordey. [illegible] received a [illegible] The tests were [illegible] having a capacity [illegible] at a pressure of 2,250 volts [illegible] flowing into the main was 6·8, [illegible] was giving approximately a sine wave. [illegible] the choker, which I understand had a weight of 80 [illegible] was 6·6. The actual current required to [illegible] combined was 2·7; this compares with the [illegible] Mr. Mordey. The energy was measured by means of [illegible] construction to a Siemens Dynamometer. [illegible] coil wound with fine wire, and connected through [illegible] with the pressure side of the system. [illegible] the pressure being measured [illegible] periodicity being checked with a

periodicity recorder, and the wave form examined through Mr. Duddell's Oscillograph. The energy measured by wattmeter in both cable and choker was 798 watts; deducting from this the C^2R losses in the choker, we get 503 watts. Mr Sparks.

A test was then made with the choker in series with the cable, when the energy was found to be 763 watts; deducting C^2R losses in the choker of 281, the energy taken by the cable was 482 watts.

The current flowing into the cable is approximately that given by the formulæ for a sine curve, and this was further checked by the curve being examined on the oscillograph. The power-factor in each of these cases was ·034, and the power lost per mile of cable at 100 periods 2,225 volts being 96 watts.

These cables have been constructed for an ultimate working pressure of 6,000 to 6,600 volts at 50 periods, and on the assumption that the power-factor remains the same when the pressure is raised from 2,000 volts to this amount, the loss of energy will increase, taking the lowest power-factor found, namely, ·034, from 96 watts, as it is at present, to 335 per mile.

Taking 0·034, the power-factor found for this I.R. dielectric, and applying it to a practical instance we find:—That if four such cables, working at 6,000 volts and 50 periods, connect a distributing centre with the generating station, distant 12·5 miles, or a total length of 50 miles of cable; the annual losses from dielectric hysteresis are as follows (if all four mains remain under pressure the whole year):—

Maximum energy delivered per cable	600 K.W.
Taking three working cables and one spare, maximum energy delivered to the distribution station	1,800 K.W.
10 per cent. load factor	= 180 K.W.
Units delivered per annum 180 × 8,760	= 1,752,000
Losses in cable at 335 watts per mile.	

(On basis of power-factor of ·034, as found by Mr. Mather on I.R. dielectric when testing with a wave form approximately sine curve.)

$$\text{Losses} \quad = 335 \times 50 \times 8{,}760 = 155{,}500$$

$$\text{Per cent. loss as above to units delivered} = \frac{155{,}000}{1{,}752{,}000} = 8{\cdot}3 \text{ per cent.}$$

If we substitute for this dielectric an (oiled) paper dielectric the capacity is reduced to one-third, and the power-factor to about ·025.

The energy lost per mile (D.H.) = 82·0 watts.

$$\text{Per cent. loss reduced to} = \frac{82 \times 50 \times 8{,}760}{1{,}752{,}000} = \frac{35{,}000}{1{,}752{,}000} = 2{\cdot}1 \text{ per cent.}$$

Under these circumstances I consider that, although Mr. Mordey has been premature in bringing forward this matter for discussion before obtaining comparative results from other cables having a different dielectric, the thanks of the Society are due to him for having pointed out the importance of capacity and dielectric hysteresis in alternating-current working, which has not had the

Mr. Sparks. attention of the bulk of our members although known to some. I am aware that by using other dielectrics a lower power-factor may be obtained; but, from tests on other dielectrics, it is clear that in the best cables the power-factor is about 2½ times the amount suggested at the last meeting by Professor Ayrton, while it is possible, as is shown by the figures I have given, to find, in exceptional cases, a power-factor which is 3½ times the amount.

In conclusion I should add that my best thanks are due to two of my assistants, Mr. Dallas and Mr. Smout, for their ready help in making the foregoing tests, and also to Mr. Gillespie, of the Westinghouse Company, for the great trouble taken in making special calibrations of the instruments used.

DIELECTRIC HYSTERESIS.

Concentric rubber cable. Capacity = ·86 m.f. per mile.
Sec. = ·15 □ „

(1) [1] Energy measured by variation of input into direct-current motor driving alternator, deducting C^2R losses in motor and alternator.

∿ Alternator.	Volts.	Volts × Amperes.	K. of Cables.	Length in Miles.	Ratio of observed cap. cur. to calculated cur.	Cos φ watts V × A	Watts lost per mile on open circuit.
100 B.T.H. K.W. Single Phase 100	2,000	46,000	8·6	10	2·1	·069	315

(2) [2] (*Tests taken by Dr. Mather*) Ganz Wattmeter from transformer Fed from net work at substation, ten miles from station.

99 Mordey K.W. Single Phase 150	2,240	15,500	4·72	5¼	1·03	·034	97

(3) Original test taken 11 months ago. Energy measured by Schallenberger Wattmeter.

100 Mordey K.W. Single Phase 150	2,040	12,300	4·72	5¼	1·02	·124	275

[1] Motor generator tests are mean of several observations.
[2] Three sets of tests taken :—
(*a*) Watts measured on cable alone.
(*b*) „ „ „ and "Ironless" choker, placed across cable ends.
(*c*) Watts measured on cable alone in series with same choker.

(4) Motor and generator. Same as No. 1. Minimum observation. Mr. Sparks.

50	B.T.H. K.W. Per Phase 2-Phase 50	2,000	11,800	8·6	10	1·16	·04	47

(5) Same as (1).

50	B.T.H.	2,000	Maximum observation.	·065	77

Dr. J. A. Fleming: Mr. Mordey has given us a plenitude of material to discuss. At this late hour of the evening, I will only touch upon one point which is one of the principal ones in his paper, viz., the measurement of the dielectric losses in a cable. Mr. Mordey tells us that his experiments showed that the true losses in 5¼ miles of a certain india-rubber insulated cable was about 2 H.P., and that the power-factor was about ·12, or 12 per cent., and these measurements, he said, were made with a recording wattmeter. He gives us no details of the experiments which he undertook to ascertain the correctness of this wattmeter. He simply says it was tested on a circuit of low power and found to be correct. I might remind him that ten years ago in this room, I brought before this Institution a Paper in which a number of measurements were recorded, taken on the Ferranti cables which had just then been laid. We were at that time interested in the large capacity current of these cables, and this current was found to be something like 44 or 45 amperes at a pressure of 10,000 volts. This corresponds to about 600 apparent H.P. The chief thing which concerned those who were connected at that time with the London Electric Supply Corporation was how much of this apparent 600 H.P. was real horse power. In those days I had not any wattmeter which would deal with these large pressures, but I remember that Mr. d'Alton, who was then the engineer-in-chief of the Corporation, attacked the problem in a very practical manner. He took one of the day-load engines at Deptford coupled to a 300 H.P. alternator, and he ran it empty and took very careful indicator friction diagrams. Then he switched on to the alternator the Ferranti cables one by one, taking diagrams in between each connection, and in that way he found that the real power taken up in the dielectric of these four Ferranti cables was about 10 or 12 H.P. I have not been able to lay my hands on the exact figures of observation, but I think I can trust my memory in this respect. One thing I am absolutely certain of is that the power-factor of these cables was not so large as 12 per cent. The power-factor we found for these cables was ·02 or 2 per cent., which confirms the figures which have been given by Mr. Sparks for certain other dielectrics. If it had been 12 per cent., or anything like the figure Mr. Mordey gives, it would have meant that 50 or 60 real H.P. were taken up in the Ferranti cables, and that certainly was not

[illegible] have not used any metal [illegible] the usual productions [illegible] suggest to Mr. [illegible] that he should [illegible] wattmeter, and in view [illegible] a dynamometer watt- [illegible] power factor, I think it would be desirable [illegible] commutatory method of a kind which I have used [illegible]. Let him take a continuous-current motor and put [illegible] and provide those with brushes so [illegible] from the armature of the motor a single-phase [illegible] current. Run that motor empty from a secondary battery [illegible] the power put into it by a potentiometer with any [illegible] he pleases. Then if we connect the two brushes to the [illegible] of the cable to be tested and any true power is taken up [illegible] it must show itself on the continuous-current side of the [illegible] by an increased power absorption, and you can make the [illegible] of the true dielectric loss in that cable with very little [illegible] with [illegible] degree of accuracy by measuring the increased [illegible] on the continuous-current side. That is practically

only a refinement of the method which Mr. d'Alton employed many years ago at Deptford to ascertain for himself as a practical engineer the real losses in the Ferranti cables. Dr. Fleming.

With regard to Mr. Mordey's wattmeter, perhaps I may be allowed to say one word before sitting down. Mr. Mordey has constructed a wattmeter in which he makes one circuit do duty as the secondary circuit of the auxiliary transformer and the movable part of the wattmeter. He gets rid in this way of the mercury contacts, but I must say that I do not see that this is any very great advantage. It has one great disadvantage over the method which I suggested eight years ago of using the auxiliary transformer separately, and that is you have no means of ascertaining, by any experiments, precisely what is the phase of the current in the movable circuit as compared with that of the impressed E.M.F. on the primary terminals of the transformer. A very little difference of phase in these two quantities affects very much the wattmeter reading when operating on a power-absorbing circuit of low power-factor. Take, for instance, Mr. Mordey's own case. He tells us that his own wattmeter showed a power-factor of 0·12. If that figure is correct, it shows that the current was about 83° in advance of the E.M.F., and he found 2 H.P. taken up in the cable. If his current had been 90° in advance of his E.M.F., it would have indicated that no power was being taken up in the dielectric. Hence this inferred loss of 2 H.P. depends entirely on the accuracy of this difference of phase of 7°, and he does not give us in his paper any proof that that wattmeter was absolutely correct to that extent. If the phase difference, for instance, had been but 3½° instead of 7°, the power absorption would have been 1 H.P. and not 2 H.P. Therefore, I think it is advisable in all these measurements of power absorption in circuits of small power-factor to check these determinations by the wattmeter by some other direct method, such as I have suggested.

Dr. W. E. SUMPNER: Mr. Mordey's paper has given rise to many interesting points, but I will confine myself to two. One is the power-factor which Mr. Mordey has alluded to; the other is a factor which Mr. Mordey has left out of account, and which, I think, is of equal importance with the power-factor in determining what his paper is intended to bring before us—namely, the relative importance of the losses which go on in the cable. Mr. Mordey has given us a number of figures which give the number of watts supposed to be lost in the cable under certain circumstances; but none of these numbers have been compared with the really important matter about the cable—namely, the load that that cable is intended to transmit. There are two factors which determine the percentage which the loss in the cable bears to the load which the cable transmits. One is the power-factor, and the other is the ratio of the capacity-current to the load-current. Mr. Mordey in no part of his paper has referred to the load-current, or to the proportion which the capacity-current bears to that load-current. If, for instance, the capacity-current is half the load-current, and if Mr. Mordey's power-factor is correct, the loss in the cable is 6 per cent. of the load. If, however, the capacity-current is one-tenth of that load-current, then, supposing Mr. Mordey's power-factor is true, the loss in the dielectric Dr. Sumpner.

Dr. Sumpner.

is only 1·2 per cent. Now what is that proportion? The proportion depends upon something which Mr. Mordey has alluded to, but which he has given no precise information about—namely, the percentage drop in volts which is allowable in the line. What is that percentage drop of volts? I ask you to assume that the drop in the line is 4 per cent. I take that figure because I know as a fact that that was the percentage loss in the cable at Deptford ten years ago. Take, therefore, the case Mr. Mordey has mentioned, of 5½ miles of 37/15 cable, run at 6,000 volts, and 50 periods. Under these conditions the capacity-current works out at 9 amperes. The load-current comes out at 77 amperes, and the current density is less than 500 amperes per square inch. The load-current is 77 amperes as compared with a capacity-current of 9 amperes. That is to say, the load-current is more than eight times the capacity-current. If Mr. Mordey's power-factor is correct, the loss in the dielectric is only 1½ per cent. of the full load; or if what I consider the power-factor is correct—namely, the number which Dr. Fleming has mentioned, ·02—the loss in the cable is only ¼ per cent. of the full load. That is to say, the cable can be run for sixteen hours at the full voltage without losing more energy than would correspond with the heating of the copper in one hour at full-load. This ratio of capacity-current to load-current is a factor which Mr. Mordey has entirely left out of his paper, and which I contend is a factor just as important as the power-factor in determining the importance of this dielectric loss.

There are other matters in reference to the choker device which I should like to allude to if I had time. Mr. Mordey has not referred at all to the loss in the choker; he has not compared it with the loss in the line. If you take the power-factors which I believe to be correct—2 per cent. for the line and 4 per cent. or 6 per cent. for the choker—the power-loss in the choker is two or three times the power-loss in the line, so that if the power-loss in the dielectric is important, the power-loss in the choker is still more important.

I pass on to consider the matter of the power-factor. Mr. Mordey has given us a number for it higher than anybody else has ever obtained, and which is quite at variance with the numbers which have been published. I need not quote them. Dr. Fleming has already mentioned his measurement of ·02, which coincides with my own results and with those of Professor Ayrton. I have not had an opportunity of testing long lengths of cable, but I have tested a large number of condensers at various times during the last twelve years, and have always found the power-factor to lie between ·025 and ·015. In the large majority of cases a figure close to ·02 was obtained. How, then, is it that Mr. Mordey has got his very high results? I can suggest several possible explanations. I cannot pretend to show the reason, because Mr. Mordey has given us no particulars of the conditions of the test. But there are two points in the paper on which I should like Mr. Mordey to give us information in his reply. He says on p. 372 that the wattmeter he used ›r testing this cable had been specially tested on a circuit of low power-·tor. I am not going to dispute that at all, but I wish to know whether ircuit on which the calibration was made took a leading, or a lagging t, and how the power-factor was tested independently of the watt-

Dr. Sumpner.

meter under test. The point I wish to make is this, that it is quite possible for a wattmeter to measure fairly accurately on a circuit of low power-factor, say ·02, if the circuit on which it is tested carries a lagging current; and yet the same wattmeter may read absolutely wrongly, and even negatively, when the circuit takes a current of the same power-factor, but a leading current instead of a lagging one. I do not think it has been noticed before that it is easily possible for a wattmeter to read negative on a condenser circuit. It is a fact that can be easily explained when the low power-factor of the condenser is taken into account. If a condenser has a power-factor of only ·02, it means that the phase difference between the pressure-current and the load-current is 88·6°. Thus the phase difference falls short of 90° by only 1° or 1½°. If, therefore, the lag in the pressure-coil is more than 1½°—and that is a very small amount—the phase difference would not only reach 90°, which would reduce the deflection to zero, but it would be actually more than 90°, so that the wattmeter would read negatively. This is not a merely theoretical point: I have tested it experimentally. I applied an alternating voltage to a condenser through a wattmeter and obtained a certain reading for the power. I then inserted a very small amount of self-induction into the pressure-coil, and the reading was reversed in direction. Now is there anything in Mr. Mordey's conditions of test which would produce a lag of 1½° or more? I think Mr. Mordey must have had in his circuit something which did that. I believe he must have had a transformer in connection with his wattmeter. At all events, I should like him to mention in his reply whether he used a transformer or not. If you use a transformer the assumption usually made, that the voltages of the primary and secondary circuits are in the same phase, is only approximately true. It is true enough for the ordinary circumstances under which a transformer is used, but there is always a difference in phase of something like 4 or 5 degrees between the two voltages, and if the load-currents are anything like the normal load-currents in the transformer, the phase difference may be considerably greater. That phase difference may produce some very extraordinary results when the converter is used for tests depending on phase. I have tried it experimentally for the sake of bringing the matter before this Institution. I have tested a condenser whose power-factor I perfectly well knew, by means of a wattmeter to which a transformer was attached. My transformer was not a toy transformer, as in Mr. Mordey's wattmeter. I used one of a commercial size and of good make, and not having more magnetic leakage than is usually found in transformers of good make. It was of 3-unit capacity, and very under-loaded. I used it under what I may call normal conditions, the full voltage on the coils, and with small currents through them. I also experimented with it under abnormal conditions, with low voltages on the coils, and currents comparable with, but less than, the full-load currents, conditions under which these effects would be abnormally large. This is the kind of result which can be obtained. The effect on the reading will depend upon the way the transformer is connected up, whether it is used in the pressure-circuit, or in the current circuit. The effect may be to increase or to decrease the reading. It may make it negative or positive. Taking

[illegible]

[illegible] and $p = 2\pi n$ where [illegible] is the tangent of the angle re[illegible] difference between the current in the pressure-[illegible] it is supposed to indicate, it will be found that—

$$\frac{\text{[illegible] reading of wattmeter}}{\text{true reading of wattmeter}} = \frac{f-a}{f} = 1 - \frac{a}{f}.$$

[illegible] if $f = \infty$ and a is greater than f the reading will be [illegible] if the reading is negative f must be less than a. More-[illegible] with frequency, being proportional to n, the correcting [illegible] of the wattmeter must also alter with n. It also alters with f. [illegible] of the pressure coil of a wattmeter may easily exceed [illegible] p, when the current frequency is 50, is 314, it follows that [illegible] be greater than f for a condenser circuit.

[illegible] There are four main points in Mr. Mordey's [illegible] choking coils to take up the capacity-current of

Mr. Swinburne

cables; measuring capacity in terms of pressure frequency and current; importance of loss of power by dielectric hysteresis; and a form of wattmeter.

As regards the placing of choking coils and condensers in parallel, the scientific, or elementary trigonometric knowledge was quite old. The idea is also old in a way as a piece of practical engineering. Condensers were sold to take up the wattless currents of Hedgehog transformers. Mr. Mordey has himself dug up a testing arrangement at the Silvertown works. This was supplied about ten years ago, and consists of two transformers giving 40,000 volts and 3 amperes, supplied by a 30,000 watt dynamo. There are two choking coils made adjustable, taking up to about 100,000 apparent watts. I had completely forgotten all about designing these until Mr. Mordey told me about them. But, in spite of these things, I think that Mr. Mordey has brought forward an arrangement which is new to most of us, though a rigid search would have unearthed primitive users. There is generally no difficulty in finding earlier examples of anything, but the device has to be re-invented before you can begin to make the search for prehistoric examples. As a matter of fact, there was a period of alternating current inventive activity about ten years ago, and much of what was done then is now buried, as it is never read, and is not, therefore, public knowledge. It requires nearly as much ability, and, in some cases, just as much, to re-invent a device as to invent it for the first time. Apparently there were many cases where the capacity current was a difficulty. Those who had to deal with it did not know what to do, and those who did know what to do did not exist, or were not there; and Mr. Mordey stepped forward and solved the difficulty.

Measuring capacity in terms of pressure, current, and frequency, is an old practice, but a practice that has been largely forgotten. My condensers in old days were all labelled in current. Though Professor Ayrton may have asked for his condensers to be graduated in this way, our practice was not the result of his suggestion. It was always the practice, and all the condensers were marked in current. The method is none the worse for being suggested by Professor Ayrton. As engineers measured the idle current of the Hedgehog transformers in amperes, it seemed the obvious thing to mark the corresponding condensers in amperes. This practice has died out, and, though a condenser cannot be accurately marked in amperes, as a rough method it is good, and, though he now disclaims it, I think Mr. Mordey did well in calling attention to it.

The loss of power by dielectric hysteresis, or whatever we like to call it, is a matter of the gravest consequence. Whether Mr. Mordey has measured it incorrectly or not I have no idea; neither do I think it matters very much, unless his measurements are so very erroneous that there is no really important waste of power in the dielectric. As the waste of power is going on always, it is a serious matter, even if small in comparison with the full load copper loss of the cable. This dielectric loss will vary enormously in different cables. Not only will rubber differ largely from paper or oil cables, but most likely different cables of the same make, and perhaps even different lengths of the

Mr. Swinburne. same cable will vary considerably. Temperature will also probably affect the loss greatly in a given case. That may, to some extent, account for the discrepancy between Professor Ayrton's and Mr. Mordey's figures, though it can hardly account for the whole difference. The term dielectric hysteresis has been applied a little rashly. If Maxwell's suggestion is at all near the truth the term is misapplied, and the loss is purely due to C^2R. For example, condensers in series with resistances, or condensers in series with some of them shunted by resistances, would show apparent dielectric hysteresis. Again, the insulators that heat most are those whose specific inductive capacities are abnormally high in relation to their refractive indices, and this would tend to show there is, so to speak, molecular conduction, or little-resistant paths of lengths of the order of the dimensions of molecules. It is probable that, if there is a demand for cables with low dielectric losses, such cables can be made. The power-factor of the condensers made at Teddington was much lower than the figure given by Professor Ayrton. It is quite possible they may have deteriorated in ten years; that is more likely to be the solution than that Professor Ayrton's measurements are wrong.

A wattmeter must be specially well designed to test loss in dielectrics. The simplest way of checking them is with a two-phase dynamo. A resistance load is put on each circuit, and the wattmeter circuits changed over so as to get two readings, so as to get the error due to the wattmeter in one direction, while the error due to want of symmetry of the dynamo cancels out. When I made things I made wattmeters, and they were checked with an old Gramme Jablochkoff machine. In spite of that Dr. Fleming's indictment is true. It was not a question of fiendish ingenuity on my part though. The instrument was designed all right, and the main coils were carried by two insulated brass pillars. The works saw no reason for insulating one end of these pillars, and removed the insulation. Hence the accusation of crime. How many wattmeters were sent out like this I do not know. The resistance to be used in series with the pressure coil is most important. We wound them with a single wire, reversing the bobbin in the lathe after each layer. This avoids self-induction, capacity, and leakage errors, and is much better than double-winding. Mr. Duddell finds that one of these bobbins is faulty, and apparently sparks inside. Whether that is a fault in make-up or an error in design I do not know, as I have not examined the coil. It is probably a fault in make up of the particular coil; but, in any case, Mr. Duddell's observation is important.

As to the particular wattmeters used in Mr. Mordey's experiments, there is no very clear account given. Mr. Baillie pointed out to me some time ago that self-induction, the ordinary error of a wattmeter, diminishes the reading on a condenser, unless it is so great as to cause the instrument to read negative watts.

Mr. Mather. Mr. T. MATHER: Mr. Mordey's paper has been useful in drawing attention to the importance of Dielectric Hysteresis losses in concentric cables. There are, however, several points in his paper which cannot pass without criticism. The chief of these points is that *a concentric cable absorbs a considerable amount of power when subjected to high*

alternating pressures, and since nearly half the paper is based upon this *alleged* fact, it demands first place in the discussion. Mr. Mather.

I may preface my comments by saying that to avoid ambiguity I shall use the expression "volt-amperes" instead of "apparent watts," and restrict the word "watts" to *real* power.

Mr. Mordey says that cables have power-factors as high as 0·124, *i.e.*, they waste, at light load, about ⅛th the volt-amperes supplied to them.

It should be noted that this number is the result of a single experiment on a single cable, and does not seem to have been repeated or checked by any independent method, and yet Mr. Mordey bases half his paper on it and works out tables of losses for cables of various capacities working at various voltages as if his power-factor 0·124 was like the "laws of the Medes and Persians." That this value 0·124 is far above the average is conclusively seen from the numbers given below in Table I.

The loss found by Mr. Mordey is so large that it would have been quite easy to measure it in several well-known ways, and in my opinion the high figures should not have been published until there was no doubt whatever about their substantial accuracy. For in effect Mr. Mordey's result condemns cables for high pressure alternate current working on the uncorroborated evidence of a single specimen. How would Mr. Mordey like all his alternators condemned because the tests on a particular alternator of some other make showed, or seemed to show, it to be a very poor one?

For, even if we grant the loss mentioned by Mr. Mordey for the particular cable, it would still have been desirable to test many other cables before drawing generalised conclusions intended to apply to *all* cables.

Ordinary wattmeters, and especially so-called "recording wattmeters," are very inaccurate at low power-factors unless special precautions are taken to ensure the current in the pressure coil being strictly in phase with the applied P.D., so that Mr. Mordey's method of measuring the loss, even though his meter, as he asserts, had been tested at low power-factors, was not a very fortunate one.

A convenient method of measuring dielectric losses is to use an "*ironless*" choker of inductance suitable for bringing the current in, or nearly in, phase with the alternator P.D., and using a moderately good wattmeter to measure the power in cable and choker. The loss in an "*ironless*" choker wound with thin wire can be found very closely by C^2R, and the differences between C^2R and wattmeter reading gives the loss in the cable. Two arrangements are possible :—

(1) *Cable and Choker in parallel* (Fig. B).—An "ironless" choker being used, as suggested by Professor Ayrton in *The Electrician* of January 18th. In this case a wattmeter suitable for high P.D.s must be used and the current coil of the instrument placed in the alternator circuit.

(2) *Cable and Choker in series* (Fig. C), as suggested by me in the *Electrician* of January 25th. Here a wattmeter suitable for low pressures will suffice and only a *low* P.D. is required. As the alternator, cable, and choker are in series, the circuit is perfectly simple. The current

Mr. Mather. coil of the wattmeter is, of course, included in the same circuit, and the pressure coil connected with the alternator terminals.

NOTE.—Since the above was written Mr. Campbell has called my attention to the fact that these methods were used by Messrs. Rosa and Smith in measuring dielectric hysteresis losses in condensers. See *Physical Review*, vol. 8, pp. 1–20. 1899. T. M.

In either arrangement it is desirable to use *electrostatic* voltmeters to show the pressure to which the cable is subjected. Results taken by these two methods as well as by measurements on the cable only are given in Table I. These numbers have been obtained by Professor Ayrton and myself, assisted by Messrs. Caine, Denton, Henry, and Mair, students of the Central Technical College, to whom our best thanks are tendered. Each value of the power-factor thus given is the mean of a large number of consistent observations, and obtained in some cases in totally different ways. There is therefore no doubt whatever as to the substantial accuracy of the results.

TABLE I. (*Mather.*)

DIELECTRIC HYSTERESIS LOSSES OF LONG CABLES.

Material.	P.D. in Volts.	Frequency.	Power Factor.	Maker.
Oiled Paper* ...	2,017	100	0·024†	British Insulated Wire Co.
Jute	2,030	71·5	0·027	Callender & Co.
India Rubber ...	2,000	105	0·028	Silvertown Co.
India Rubber, County of London Cable	2,230	100	0·029†	" "

MR. MORDEY'S EXPERIMENTS.

India Rubber, County of London Cable	2040	100	0·124	Silvertown Co.

It is significant to notice that, according to these results, Mr. Mordey's tests on the County of London Cable gave *a power-factor more than 400 per cent. too high.*

The figure 0·029 obtained in our tests on the same cable is the mean of sixteen separate experiments made in the three different ways shown in Figs. B, C, and D, none of which differed from the mean 0·029 by more than 1 in the third decimal place. Hence it seems to us that Mr. Mordey's results are very inaccurate, and should not be applied in any case.

Turning now to the secondary parts of the paper, I may remark that there is no novelty in measuring capacities by alternating currents

* Low pressure cables tested at over 2,000 volts.

† Tested in three different ways with very accordant results.

as described on page 366, nor in supplying the "idle" currents of cables by inductive coils or chokers (pages 377 and 378). Both have been common knowledge for the past ten years. Mr. Mather.

Mr. Mordey's treatment of capacity measurements is very unequal in the detail given. For example on page 365 he tells "engineers who have not hitherto considered the subject," that the unit of capacity is

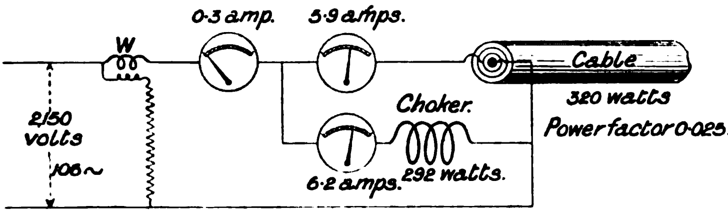


FIG. B.

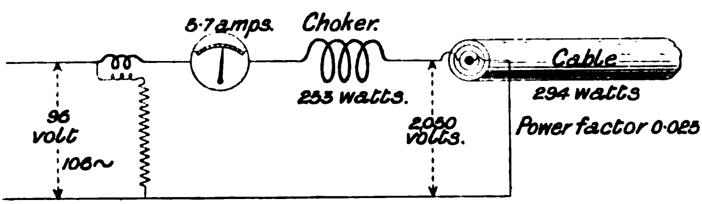


FIG. C.

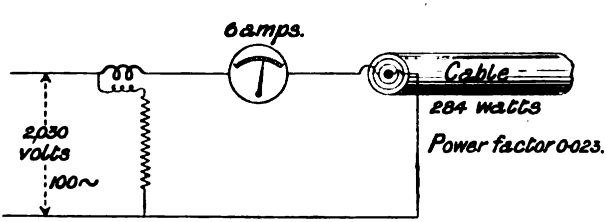


FIG. D.

the farad, and that it is too large for practical purposes. He then states that "the charging current for a cable is easily found if we know the capacity of the cable," and gives the formula,

$$\text{Charging current in amperes} = \frac{\text{volts} \times \text{periods per second} \times \text{microfarads} \times 2\pi}{1{,}000{,}000}.$$

No caution appeared in the slip proof given out at the meeting (except the obscure note on page 379) to remind these engineers that the current taken by a cable depends very largely on the *wave form of the alternator* and on the load under which the machine is working.

Then followed the statement that *one microfarad takes 0·6283 amperes at 2,000 volts 50* ∿ and a suggestion that station engineers should have ammeters graduated in microfarads. From the remarks with which Mr. Mordey prefaced the adjourned discussion, it is evident that since the reading of his paper on the 10th of January he has found out

Mr. Mather. how misleading this method for measuring capacity may be, for he now acknowledges that errors of two or three hundred per cent. may arise.

How puzzled the poor engineer, who requires to be told about the unit of capacity, would be, in making capacity measurements by such a method, to find that a particular cable had all sorts of capacities depending on the time of day at which his measurements were taken! For (as Dr. Fleming has well shown in the case of the large alternators belonging to the City of London Company) the wave-form of the pressure supplied depends on which alternators are being used, and on the load under which they are working.

The differences in capacity-current as dependent on wave-form is well illustrated by the results shown in Tables II. and III., which were taken on condensers of some 50 microfarads.

TABLE II. (*Mather.*)

CAPACITY CURRENTS AS INFLUENCED BY WAVE-FORM.

Machine used.	Frequency n	Current at 100 Volts. A.	Capacity from $F = \frac{A \times 10^6}{2 \pi n V}$ microfarads.	True capacity in microfarads.
Ferranti Alternator... ...	100	3·0 amps.	47·8	49·4
Pyke and Harris Alternator	100	4·6 "	73·2	"
Wenstrom Converter ...	43	1·18 "	43·7	"
" " ...	58	1·69 "*	46·5	"
Gramme " ...	25	0·7 "	44·5	"

NOTE.—The Ferranti alternator has an E.M.F. wave nearly of sine form, whilst the Pyke and Harris machine has an E.M.F. wave very peaked.

TABLE III. (*Mather.*)

EFFECT OF LOAD ON CAPACITY CURRENTS.

Pyke and Harris 6-KW. Alternator, run at constant speed and constant P.D.

Frequency n.	Load-current in amperes at 100 volts.	Capacity-current at 100 volts. A.	Capacity calculated from $F = \frac{A \times 10^6}{2 \pi n V}$ microfarads.	True capacity in microfarads.
100	0	4·6	73·2	
"	0·6	4·5	71·6	49·4
"	1·2	4·35	69·3	"
"	3·5	3·95	62·9	"
"	7·0	3·6	57·3	"
"	13·5	3·3	52·5	"
"	30	3·2	50·9	"

* Machine sparking a little at commutator.

Mr. Mather.

These tables show capacities differing by 58 per cent. according to the alternator used (Table II.), and with the same alternator differently loaded by putting lamps in parallel with the condensers, a change of nearly 50 per cent (Table III.). These are by no means extreme cases.

In connection with the effect of wave form on the supplying of capacity current by chokers, I may mention that with the first method used at Prescot, on Tuesday last (see Fig. B), when the choker and cable were in parallel and the pressure supplied direct from the machine, we could not reduce the alternator current below 3 amperes when about 6 amperes went through the coil and about 6 amperes into the cable, no matter at what frequency the alternator was run, the machine being a Mordey's so-called "sine wave" alternator. But when the 2,000 volts supplied by the alternator was first transformed down to 100, and then up again to 2,000, and a choker also inserted, the current supplied to the arrangement shown in Fig. B went down to about 0·3 amperes at 106 ∿. Hence it seemed that the Mordey alternator did not give a pure sine wave, and it was necessary to insert the transformers and choker to reduce the higher harmonics. Mr. Mordey's own experiments given on page 378 of his paper point to the same conclusion.

On pages 379 and 381 the subject of chokers for supplying the idle currents to cables is dealt with, and the design of a choker given. In my opinion the use of iron in a choker for such a capacity is a mistake. The late Mr. J. E. H. Gordon in the early eighties showed, at Paddington, how *not* to make chokers; and a few years later Mr. Swinburne brought out a greatly improved choker in the shape of his Hedgehog transformer. He opened the magnetic circuit, and used only a small amount of iron; and yet Mr. Mordey now describes a choker containing a large amount of iron and a power-factor as large as 0·041 for one of 12,000 volt-ampere capacity at 100 periods. Surely this is a serious retrograde step. It is generally recognised that a closed magnetic circuit in a choker is very wasteful of power, for the power-factor is usually of the order 0·6 to 0·7 for transformers on no load. Opening the magnetic circuit, although it necessitates rather more copper, greatly reduces the power-factor, and the more open the magnetic circuit the smaller the power-factor becomes. The logical conclusion to which these considerations lead is *Remove the iron altogether, and the power-factor is reduced to a small value.* The numerical magnitude of the power-factor depends on the amount of copper put in the coil, and may, in fairly small chokers containing about 100 lbs. of copper, be reduced to about 0·02 at 100 ∿ and 12 to 15 thousand volt-amperes.

On the table before you is the choker that has been used in most of the tests on dielectric hysteresis recorded in Table I. It contains 81 lbs. of No. 14 copper wire, has a total weight of 92¼ lbs., an inductance of 0·53 henry, and a power-factor of 0·021 when warm.

Mr. Mather.

TABLE IV. (*Mather.*)

COMPARISON OF 12,000 VOLT-AMPERE CHOKERS.

	Mr. Mordey's Choker.	"Ironless" Choker.	Ratio
Total Weight	260 lbs.	92·5 lbs.	2·8
Weight per kilo-volt-ampere	22 „	7·7 „	2·8
Loss at 2,000 volts, 100 $\sim$	500 watts	250 watts	2·0
Power-factor „ „ „	0·041	0·021	1·9
Cooling surface per watt ...	1·2 sq. in.	1·7 sq. in.	0·7

A comparison of the "ironless" choker with the one described by Mr. Mordey is given in Table IV., from which it will be seen that in the ironless choker we have one of *not much more than one-third the weight of Mr. Mordey's, having half the loss, about half the power-factor, and a greater cooling surface per watt*, whilst the simplicity of construction is such that the cost must be considerably less.

In predetermining the winding, calculation gave the number of turns as 1,367, and after winding it was found that 1,348 turns gave the required inductance. You will, therefore, see that calculation, taken only to a first degree of approximation, gave a result true to within 1½ per cent.

Much more might be said on the subject of chokers of fixed and variable inductance, but these matters I hope to bring forward in a separate paper, on which I am now engaged.

My sincere thanks are hereby tendered to the following gentlemen for the facilities and assistance so kindly given :—Professor Ayrton; Mr. Duddell; Messrs. W. and R. K. Gray, Stuart A. Russell, and Mr. Grafton, of the Silvertown Company; Messrs. T. and J. Callender, and Mr. Goodman, of the Callender Cables Company; Mr. G. H. Nisbett, of the British Insulated Wire Company; Messrs. C. P. Sparks, J. Smout, and J. H. Butler, of the County of London Brush Company; Messrs. W. Cramp, C. H. Hainsworth, T. L. James, W. Templeton, and T. R. Sowerbutts, my colleagues at the Central Technical College; and Messrs. Ablett, Blennerhassett, Duncalfe, Fasola, Griffin, and Harrold, students of that Institution. My best thanks are also due to Mr. Cramp for the clear and effective way in which he communicated my remarks to the meeting.

(*Added February 23rd*, 1901). Amongst the numerical errors occurring in Mr. Mordey's paper the following may be particularly mentioned :—(*a*) At the bottom of page 372 the C^2R loss in the cable is given as 13·4 watts. Taking the data given on page 373 respecting the resistance of the cable, I calculate that the copper loss in the cable when the capacity current is 6 amperes amounts to 35·4 watts. (*b*) On page 383, after referring to the disproportionality between the large hysteresis loss and the insignificant loss due to leakage, Mr. Mordey goes on to say that the dielectric hysteresis loss is as much as the leakage loss would be if the insulation resistance between inner and outer conductors was 9,524 ohms, instead of 2,500 megohms. Surely

Mr. Mather.

the disproportionality is not quite so great. Mr. Mordey has in this case neglected his power-factor 0·124, for it is evident that 10,000 volts on 9,524 ohms would waste 10,500 watts, whereas the power wasted is given as 1,296 watts. The resistance corresponding with the waste 1,296 is 77,000 ohms, and not 9,524.

Further, insulation resistances are usually given after one minute's electrification; and it is well known that the insulation resistance decreases as time of electrification decreases. When subjected to alternating pressures of 50 $\sim$, the time of electrification to be considered is of the order $\frac{1}{100}$ of a second, and the insulation resistance of a 2,500 megohms cable would be considerably less at such short times of charging. Experiments made some years ago showed that rubber cables after short periods of electrification had resistances from one-third to one-fifth of their value after one minute's electrification. The disproportionality would, therefore, be more fairly expressed by, say, 600 megohms to 77,000 ohms, instead of by 2,500 megohms to 9,524 ohms.

Mr. O'Gorman.

Mr. M. O'Gorman: Whether Mr. Mordey's test or Professor Ayrton's of the energy losses on the London and County Company's cable was the more accurate cannot be in any way verified by tests on any other cable or condenser, for the following reason: The work done on any commercial dielectrics upon which any published tests or experiments which I have been able to find have been made, varies as the square of the potential difference established between the two sides of the dielectric in question.

These results have been endorsed and quoted by Steinmetz, and independently examined by Arno, Threlfall, J. Sahulka (Wiener Sitz Ber. vol. 102), and recently quoted by Dr. De Hoor in the *Electrician* of February 8th.[1]

If this is so, the energy lost depends upon the distribution of potential within the thickness of the insulation, and this in turn depends upon the grouping of the materials in the various layers of any individual cable.

Taking a 37/15, which was the size of cable Mr. Mordey tested, and giving it, for the sake of example, the subjoined arbitrarily chosen thicknesses of dielectric (rubber) we get a curve of potential which is approximately according to Fig. E for continuous and Fig. J for alternating pressures, whereas if the dielectric were perfectly homogeneous the potential would be somewhat according to Fig. F.

The gradient or slope of potential is at every point in Fig. E indicated by the height of the ordinate of Fig. G, whereas in a uniform dielectric the gradient at every point is approximately according to Fig. H for both continuous and alternating pressures. The method of obtaining these particulars and of verifying them experimentally I hope to show on a future occasion.

[1] Under the entirely uncommercial conditions of a dielectric which has been elaborately freed from air and moisture, Mr. Threlfall has succeeded in reducing the energy wasted in a condenser to a very large extent, but he has also, by making an imitation commercial dielectric with graphite and paraffin, endorsed the above contention that the energy varies as the square or 1·6th power of the E.M.F.

Mr. O'Gorman.

If we add together the work done on each element of thickness on a homogeneous cable and add together the work done on each element on the above heterogeneous cable, it will be seen that the ratio of energy lost may be as 6 to 1 in one of the above cases.

These considerations explain how it is that Professor Ayrton found

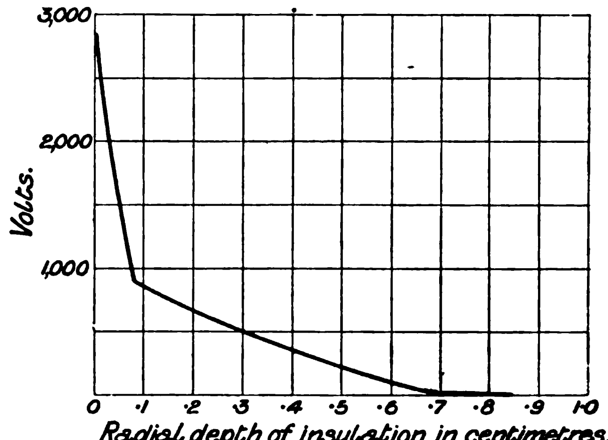


FIG. E.

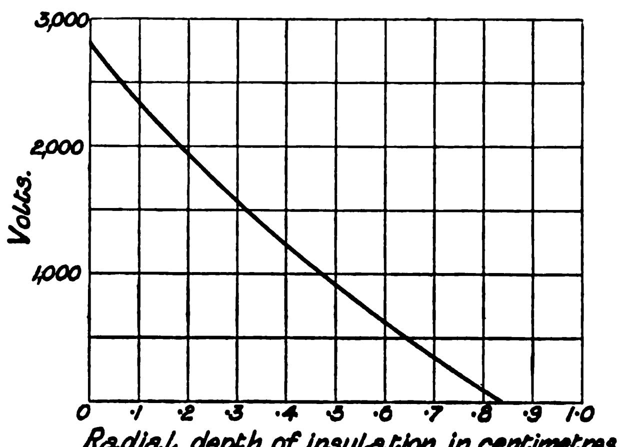


FIG. F.

power-factors for different cables varying from ·024 to ·033, and there seems no reason why the power-factor should not reach and exceed Mr. Mordey's figure in many cables which are actually in use and where no idea of such losses is entertained by their users.

These considerations also explain why the loss is greater when the curve of the alternating voltage is more peaky than a sine curve, for the

Mr. O'Gorman.

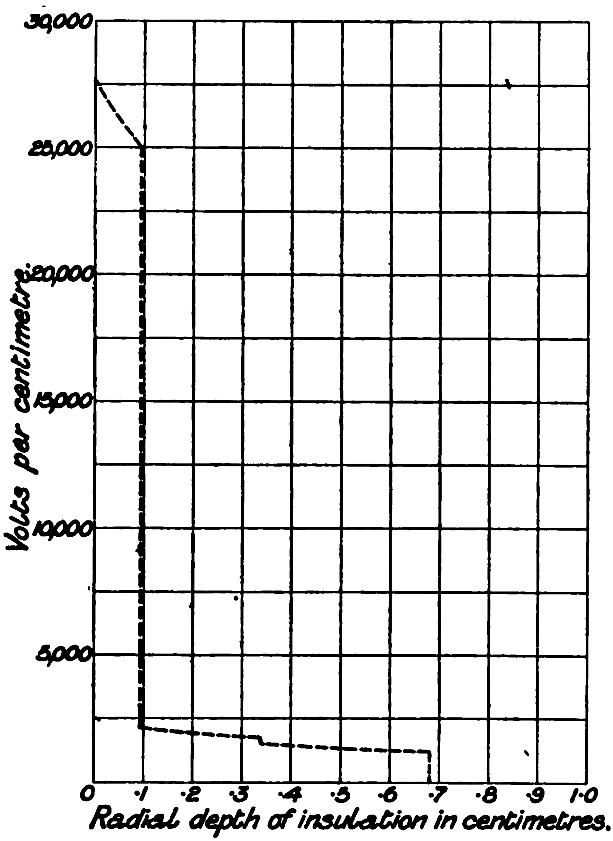


FIG. G.

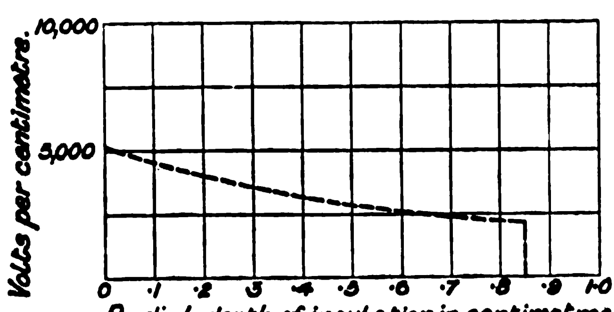


FIG. H.

Mr. O'Gorman

steepness of the gradient depends on the maximum pressure at ordinary frequencies. There is, in fact, no reason why a cable dielectric should not on some occasions actually be broken down in one layer while being perfectly sound in another. This form of breaking down would not lead to a disruptive spark right through the dielectric, but would lead to a phenomenon similar to a brush discharge from a wire in air.

When Mr. Mordey at the end of the discussion quoted Mr. Kapp as saying that certain cables that he had tested had become appreciably hot to the touch, one could not but surmise that a partial breakdown on the above suggested lines was occurring in the cables in question, and that this partial breakdown had probably occurred in the layers which were close to the lead sheath, the cable as a whole not having been raised in temperature by two or three degrees Fahr., but only the outer layers.

I would like to suggest the official abolition of the phrase "apparent

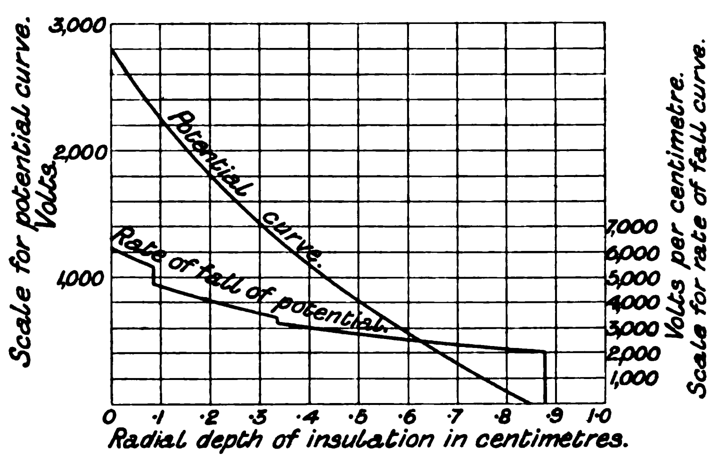


FIG. J.

watts," and the worse, because longer, phrase "phantom energy," and to suggest instead of an apparent watt not the name a "phantom erg," which it is not, but a "vamp," as an abridgment of "voltamp," which it is.

Mr. Gray.

Mr. W. E. GRAY: I have not much to say, and shall occupy little of your time. We have heard Mr. Mordey, and I, at least, have to thank him for the information he gives, by which I hope to profit considerably. Professor Ayrton and Mr. Sparks have also given us papers which are very interesting, as they appear together to summarise general knowledge up to date. There is one point on which I differ with Mr. Mordey; that is the question of capacity on a concentric cable when earthed. (In such a case I do not think the capacity of the outer can be ignored altogether, when, according to Board of Trade rules, it is earthed at one end only.) We have heard during the discussion that instrument makers do not know how to make instruments; that dynamo

makers do not know how to make dynamos to work efficiently on a distribution network; and, finally, that cable makers cannot design cables. In fact, the discussion, instead of being concentrated on the question of importance or non-importance of hysteresis, has resolved itself into a three-sided duel. I do not propose to join in this. The point I wish to speak on is that, when dealing with the question of capacity of dielectrics, little should be taken for granted, as there are many things to be borne in mind. Although in this discussion rubber and paper have been referred to as dielectrics, and a specific capacity for each has been quoted, this is not necessarily correct; indeed, it varies very considerably according to the different processes adopted in manufacture. Of this there is no doubt; consequently any statements which presuppose a definite specific capacity for india-rubber or paper as manufactured are misleading and inaccurate if too widely applied, and I think when making statements here our professors should differentiate, as the adoption of a "rule of thumb method" when dealing with such matters is not worthy of professors. In this connection I think that without going too far I could undertake, within certain limits of course, to make rubber cables of any capacity. In the same way no doubt other makers using different dielectrics could also make any reasonable variation of capacity that might be required; but it is another matter to make a cable whose specific capacity will not vary. Even in cases where the insulation resistance remains quite constant, often the capacity will vary to a considerable amount when a cable is ageing. Thus when manufactured a cable may have a specified insulation and capacity, and when tested some years after, although the insulation resistance may not have varied, the capacity in many cases will have changed considerably. It is also possible to make a cable which will not, at least for some time, vary either in insulation resistance or capacity. If an unvarying capacity is required, are engineers in central stations—those that design machinery or those that teach how to design—prepared to say what capacity is wanted to meet their requirements? Apparently not yet. With regard to dielectric hysteresis, many of those who have spoken differ entirely; even Mr. Mather differs from Professor Ayrton. Look at the results obtained when Mr. Mordey tested one form of cable and Professor Ayrton with Mr. Mather tested another form of cable. Mr. Gray.

Professor AYRTON: It was the same cable. Professor Ayrton.

Mr. W. E. GRAY: Mr. Mordey tested one form of cable; you tested another form. Mr. Gray.

Professor AYRTON: But it was the same cable. Professor Ayrton.

Mr. W. E. GRAY: I am talking in another sense. You tested at Silvertown a certain form of cable entirely different in construction from the one tested by Mr. Mordey and got totally different results, but you also got another different result when you tested the same cable as Mr. Mordey; and, in fact, your two tests on the rubber cables did not agree. How do you know that you did not make an error in your tests? Is the difference due to the fact that you did not take into consideration the fact that the forms of cable were different? Mr. Gray.

Professor AYRTON: The last one is the very cable Mr. Mordey tested. Prof Ayr

Mr. Gray.

Mr. GRAY: Yes; but you got a very different result from the other, and your results don't agree.

[*Communicated*]: I certainly understood that Professor Ayrton first held that the power-factor was so small as to be negligible, and Mr. Mordey pointed out that it was important. I cannot say whether or not Mr. Mordey exaggerated the importance; but, in any case, as shown by the figures given by Mr. Mather, it was not negligible. Professor Ayrton and Mr. Mather got two very different results with the same dielectric; it appeared to me, therefore, for that reason and others, that the difference might be accounted for by the difference in the forms of the cables tested by these gentlemen, the one at Streatham and the other at Silvertown.

Professor Threlfall.

Professor R. THRELFALL: Mr. Mordey has pointed out in his paper that he was able to measure the loss in a certain cable, and that loss, he says, is a dielectric hysteresial loss, on the grounds that the conductivity and leakage losses could be ascertained and allowed for, and that both these sources of loss were found to be negligible. With regard to the copper losses, no doubt Mr. Mordey is well qualified to speak, and no doubt he is also well qualified with regard to the leakage losses; but my experience of leakage losses has been that they are exceedingly subtle things. For instance, you may take a film of sulphur which is composed partly of crystallised and partly of amorphous sulphur, and you may obtain any resistance within a range of 10 to 1 according to the length of time you apply the voltage, according to the direction in which you apply the voltage, and according to the magnitude of the voltage. Therefore there is no such thing in the true ohmic sense as the resistance of sulphur, or of similar dielectrics. In short, the "resistance" of a dielectric is only definite under specified conditions of electric intensity in the dielectric, direction of that intensity, and whole previous history. It seems to me, therefore, that there is, theoretically at all events, an absolute bar to the establishment of Mr. Mordey's position as to the nature of the losses observed. I believe there has been a great deal too much readiness to talk about dielectric hysteresis. In 1892 Mr. Steinmetz, by means of a wattmeter which may or may not have been correct, measured the loss in a condenser, and he referred to this loss as due to dielectric hysteresis. Mr. Arno, an Italian who has done work on the subject, referred to the effect of dielectric hysteresis as having been established by Steinmetz. I need hardly say from what has passed this evening that it was not established at all. He might as well have deduced "original sin" as have deduced dielectric hysteresis from the experiment from which conductance loss was not really eliminated. There is generally, and perhaps universally, a tendency to associate the heating losses in a condenser supplied with alternating currents with those phenomena which have long been known to electricians as phenomena of absorption, such as are observed when a condenser is being tested for capacity by slow-speed methods. These two causes of loss, as I was able to show in the years 1896 and 1897 (*Physical Review*, 1887), are only approximately related, at all events if the alternating current has a frequency of 30 or 40 a second. With regard to the absorption losses, so-called, $\frac{9}{10}$ths of the

Professor Threlfall.

are, I am perfectly certain, due to spreading over the surface of the dielectric beyond the armatures and to air discharges, as suggested by Lord Kelvin, between the armature and the dielectric surfaces. My attention was first called to this by finding that the residual charge in sulphur, in quartz, and in mica, all of which give exceedingly low residual charges, can be made to give very much lower ones by drying them over phosphorus pentoxide; and I also found, in making measurements by the rotating field method of the dielectric losses in solid dieletrics, that it was necessary to dry the specimens over phosphorus pentoxide for days before a constant result was obtained. Moreover, the method of the rotating field possesses the merit of avoiding the introduction of sharp variation in the electric force at the surface of the dielectric, such as occurs at the edge of a condenser plate, and consequently gives results which depend less on the state of the surface of the dielectric than do the usual methods. Taking all these things into account, I believe that we ought not to talk about dielectric hysteresis loss in such a case as that of the sheathing of a cable where there may be air or moisture, and where the slightest trace of air or moisture will, as Mr. Sparks has said, increase the loss to an enormous extent. If you make a condenser from which air and moisture are entirely excluded—and it may be done—then you can reduce the losses to a figure which I hardly like to mention, so small is it. It is something nearer 0·1 than 1·0 per cent., at least with a mixture of paraffin oil and vaseline. To obtain this result pure cellulose paper dried for some time at a temperature of 130 C., must be employed, and the dried paper must be further treated in the dielectric at that temperature and under a vacuum. It is the last thousandth or millionth of percentage of the air and moisture originally present that makes all the difference between whether the loss in a condenser used with alternating currents is large or small. That is a fact which I published in America and in this country, and apparently I have utterly failed to reach my audience. I do not believe there is a man in this room except myself who is aware of it. Mr. O'Gorman holds up his hand, but I cannot include even him, as he misquoted me just now. I understood him to say that the loss in a dielectric—according to me—is in proportion to the square of the electric force.

Mr. O'Gorman.

Mr. O'GORMAN: I think you stated that to be the case in the *Physical Review* in 1897.

Professor Threlfall.

Prof. THRELFALL: No. The losses are proportional to the internal electric force raised to some power between 1·5 and 1·9. In ordinary dielectrics the index only reaches the value of 2 when you make a dielectric purposely heterogeneous by mixing graphite in paraffin to such an extent that you probably get conductivity losses. With regard to the table which Mr. Mordey has so valorously prepared, I am afraid he is foredoomed to disappointment if he expects it to apply generally. My reason is this. I will merely mention this fact. I have taken a ladle full of melted resin and from it I have cast spheres for investigation. The spheres have been cast one after the other, the resin not even requiring to be re-heated between the separate castings; and among the spheres so cast the dielectric losses have varied by some hundred per cent. Summing up the matter we may say—(1) There is such a

Professor Threlfall.

thing as "dielectric hysteresis," *i.e.*, when a small volume of dielectric is carried round a cycle of electric force, some of the energy of electrification is dissipated. (2) The laws connecting this dissipation with the electric force, rate of passing through the cycle, &c., are perfectly fixed and definite for each volume of dielectric. (3) It is practically impossible, however, to find or prepare two small pieces of any given dielectric which shall have even approximately the same properties in regard to the dissipation of energy. (4) The cause of the dissipation of energy is not known; but Maxwell's suggestion as to heterogeneity being possibly the cause, is shown by experiment to be most probably an insufficient explanation.

[*Communicated*]: Though properly unwilling to take up the time of the meeting with such a matter, I feel that I must add a note in regard to a criticism of a statement of mine about condenser efficiencies which has been made by Messrs. Rosa and Smith (*Phil. Mag.*, vol. xlvii. p. 19).

In the *Physical Review*, vol. iv. p. 458, and for the sake of illustration merely, I compared the efficiency of one of my specially prepared condensers with the efficiency found by Mr. Bedell and others for a waxed paper condenser. The comparison I made was based on the relative rise in temperature in the two condensers working under different but specified conditions. To make this comparison I assumed that the condensers were roughly similar both as regards capacity for heat and for electricity. Messrs. Rosa and Smith point out that unless the ratio of the thermal to the electric capacity was the same in both cases, the reasoning would be fallacious; and they then go on to show that, assuming this ratio to have the value nine in one case and one in the other, the performance of the condenser would be less satisfactory than I stated.

My reason for referring to the matter at all is that I think that anybody reading Messrs. Rosa and Smith's paper would come to the conclusion that the losses reported by me were less than they ought to have been; in fact, that I underestimated these losses, and also by implication that it is not possible to make as good a condenser as I have made. This would be a misfortune, and on this account I wish to explain how the matter really stands. I need hardly say that of course I entirely agree with Messrs. Rosa and Smith's criticism, which is perfectly just, and the cause of the misunderstanding is entirely on my side. While writing the passage in question I thought it would be interesting to make a rough comparison with the results attained by Mr. Bedell, but I certainly never expected such a comparison to carry weight. I *had measured the loss in the condenser referred to by a thermal method* very similar to the one afterwards used by Messrs. Rosa and Smith and for the same reasons, but I had not considered the matter of sufficient importance for publication. Indeed the loss was so small that my calorimetric method would have been inaccurate, and the loss was ascertained by comparing the temperature rise in the case in question with the rise under similar circumstances of a less well-made but otherwise similar instrument which had itself been subject to investigation by the calorimetrical method. The result of this comparison was to show that the condenser wasted less than 0·1 per cent. of the energy it

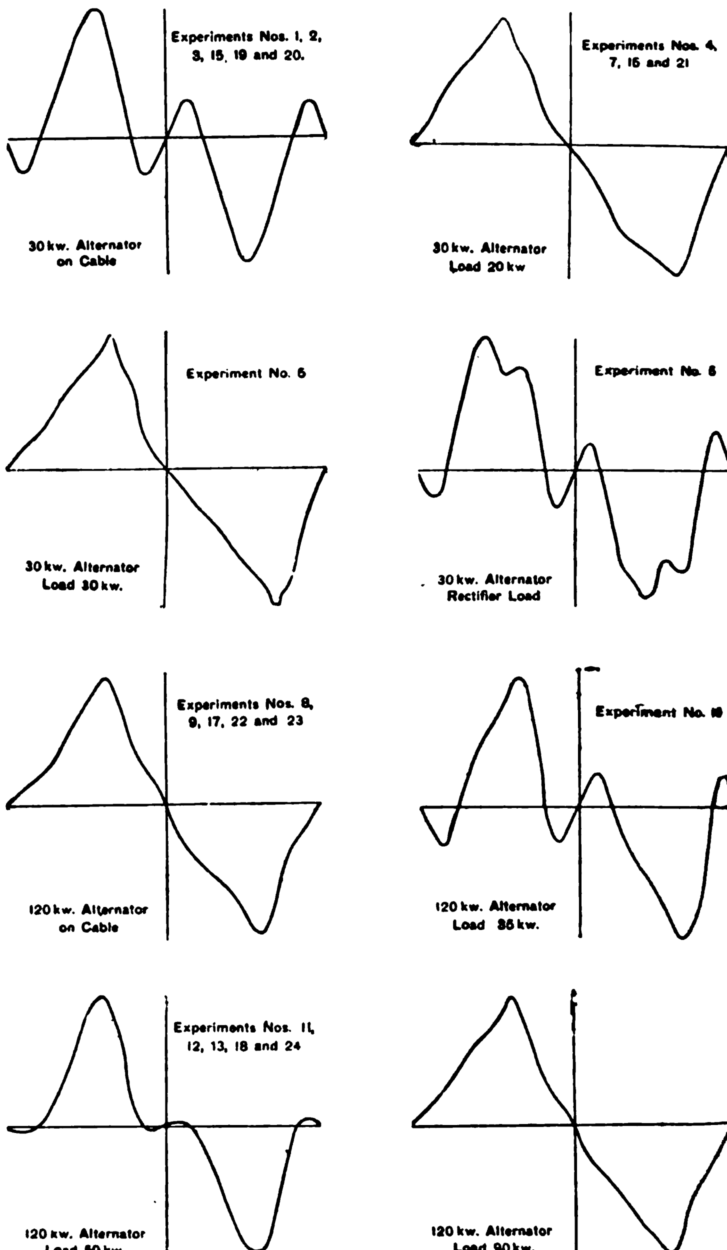
received: how much less I don't know. Now, in writing the passage which is the subject of discussion, I found that the loss roughly (and, as Messrs. Rosa and Smith show, illegitimately) deduced from the comparison with Mr. Bedell's work came out at 0·05 per cent., and, this being about the value which I already knew to be correct, I neglected to go on to state, as I ought to have done, that I had made an independent measurement. I consider it well worth knowing that it is possible by following the instructions (given in my little book on Laboratory Arts) to prepare a condenser which will not heat appreciably on alternate current circuit, even though the condenser, when tested by charges of long duration, shows the phenomena of absorption. Professor Threlfall.

The condensers I have made have been constructed from filter paper: I have not until recently had command of the special papers which are used in making artificial cables, and which, if they will stand the treatment, I should expect to be even better. As a dielectric I know nothing better than vaseline or a mixture of vaseline and paraffin. This is more convenient than burning-oil, because it allows one to attain a temperature of 130 C. without volatilising much. No doubt, however, any good hydrocarbon oil satisfying this condition would do equally well. It is, in general, better to use two thin sheets of paper than one thick one, in order that if there is a fault anywhere in one sheet we may have the other to depend upon. The purest white filter paper has been found to do well; but the beginning and end of the matter is getting rid of air and moisture.

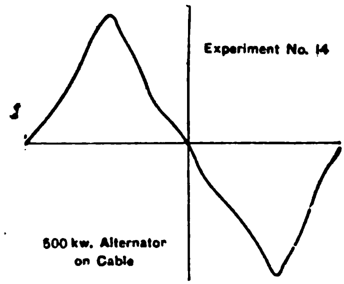
Mr. T. H. MINSHALL: The figures in the accompanying tables show the results of some experiments made at Croydon since the previous meeting. They were devoted more particularly to ascertaining three points: (1) The effect of wave form on the quantity of the capacity-current; (2) The power-factor of the cables with different wave forms; and (3) The effect on the capacity-current of a small load at the end of a cable. We started with the idea of proving that Mr. Mordey's figures were wrong and ever so much too high. The first thing I did was to take the various wave forms of three different sizes of machines under various conditions of load, and they gave the various currents shown in Table I. The machines were all by the same maker, and the various wave forms shown in Fig. K have been sketched from observations on the oscillograph. As regards the watts, the wattmeter readings probably vary slightly with different wave forms; but there can be no doubt whatever with regard to the current readings, as they were taken with Siemens dynamometers. The impossibility of calculating capacity by the usual formula is shown by the fact that on machines of the same type we got capacity-currents varying from 2·67 amperes to 1·85, which latter is the capacity-current given by the 500-kw. machine. The differences shown, confirm what was, of course, well known, and what Mr. Mordey has already mentioned—namely, that it is impossible to make a commercial test of capacity unless you have some means of making perfectly certain that you have a sine wave. As a matter of fact, it is only by using a copper-cored machine, such as a Mordey alternator, and a good cable that you can possibly get a good wave. That probably accounts for the fact of Mr. Mordey's figures agreeing Mr. Minshall.

Mr. Minshall.

WAVE FORMS APPLIED TO CABLE.



Mr. Minshall.



approximately with the calculated capacity. If you have machines like those at Croydon you do not get a sine wave, as you see. The next thing we did was to measure the power-factor of the cable. It has been said by Dr. Sumpner that a cable cannot be got with a power-factor of ·1, as the author found. I do not want to say anything as to how that figure has been got by the author, but I am inclined to think that the discrepancy between the statements of Professor Ayrton and Dr. Sumpner, on the one hand, and Mr. Mordey's and my own figures on the other, is due to the fact of the variation of insulation resistance and the absorption factor of the dielectric mentioned by the last speaker not being taken into account. The cable to which these figures refer has a low insulation resistance. Mr. Mordey did not mention what his insulation resistance was; but the condensers used by Dr. Sumpner probably have an insulation resistance of many hundreds of megohms. As the dielectric hysteresis is believed to be a function of the insulation resistance, the discrepancy is not surprising. As a matter of fact, these watts measured differ according to the wave forms. The readings on this cable were taken with two Swinburne wattmeters, and also with two Thomson's inclined coil instruments. All the readings in the first table have been checked, and these four instruments agree within 3 per cent. One wattmeter has been specially checked upon low power-factors at the Central Institution, and the agreement of this instrument and the other three on inductive and non-inductive load is remarkably close. Of the four instruments, half the tests were taken with a 20,000-ohm coil in the shunt and half with a bank of lamps. We got a power-factor of 0·1, due possibly partly to the fact of our cable having an insulation resistance of only half a megohm. This is not so. I have since obtained the same results with an insulation of 50 megohms. It has been used for two years as a 2,000-volt cable. As a matter of fact, makers are making very much lower insulation resistances than formerly. The consulting engineers are trying to keep them up; but the cable-makers, of course, are trying to keep them down. Once you get them lower, you avoid a lot of trouble. With our 5,000-volt cable, by having much lower insulation resistance than before, we have avoided a lot of trouble. I see no reason to doubt any of these figures; we have checked them in every possible way by many methods. I have had the assistance of Mr. Duddell, and we have gone most carefully into the question in every

Mr. Minshall.

way. As a further check I have made the test which Dr. Fleming suggested, viz., using a motor-generator. I took particular steps to avoid changing the efficiency of the machine due to the low power-factor of load by loading up the motor generator with a large non-inductive load. Then I measured the increase in the D.C. watts taken when the cable was switched on. The increase was almost exactly the mean of the results shown in Table I. Then, with regard to the other question of running a machine, I entirely agree that capacity in cables is a very good thing indeed in power systems, and the capacity has helped us to improve the power-factor of arc lamps. I have taken the same machine running on each one of the various wave forms, and find that a very small load at the other end of the cable reduces the capacity-current taken by the cable very considerably. This is seen in the lower part of Table II. Another experiment was undertaken to show that with a certain wave form and suitable capacity and non-inductive load at the other end of the circuit, you may actually have *fewer* amperes entering in the cable than you get out at the other end. That is not a kind of perpetual motion, but merely a phase shifting—that is to say, under certain conditions by increasing the capacity in the cable reduce the current entering in.

Table III. shows another advantage due to capacity—*i.e.*, the marked decrease in exciting energy required for the machines.

Mr. Minshall.

TABLE I. (*Minshall.*)

Losses in V.B. Concentric Cable.

Length of Cable, 7,460 yards.

Section of Conductors, 0·10° Concentric.

Total Insulation Resistance, 0·5 Megohms.

No. of Experiment.	Machine running.	Input at Station end of Cable.			Watts wasted in Cable.			
		C.	Watts.	P. F.	C^2R due to Capacity Current (approx.)	Leakage Losses (approx.)	Dielc. Hysts. Losses.	
1	30 kw. Alter.	2·44	459	0·092	14	8	425	No load on end of Cable.
2	"	2·35	489	0·101	13	8	468	
3	"	2·37	505	0·103	13	8	484	
4	"	2·18	459	0·102	11	8	428	
5	"	2·03	420	0·100	10	8	402	
6	"	2·41	499	0·101	14	8	477	
7	"	2·10	431	0·100	11	8	412	
8	120 kw. Alter.	2·14	388	0·088	11	8	354	
9	"	2·07	459	0·108	10	8	441	
10	"	2·67	551	0·100	17	8	526	
11	"	2·34	367	0·076	13	8	334	
12	"	2·28	469	0·100	13	8	451	
13	"	2·29	507	0·108	13	8	486	
14	500 kw. Alter.	1·85	384	0·101	9	8	367	

Notes:—Swinburne Wattmeter (*a*) with Potentiometer consisting of lamp bank.
Swinburne Wattmeter (*b*) with 20,000 Resistance direct on 2,000 volts.
Thomson Wattmeter, No. 9,608 with Potentiometer.

Mr. Minshall.

TABLE II. (*Minshall*).

No. of Expt.	Machine Running.	Input at Station End of Cable.			Output at Far End of Cable.			Watts wasted in Cable.				
		C.	Watts.	P.F.	C.	Watts.	P.F.	Total.	C^2R Useful Current.	Difference.	Leakage Losses.	
*‡15	30 kw. Alter.	3·66	5,250	0·70	2·22	4,680	1·03	570	18	552	8	Non-Inductive Load on End of Cable.
*‡16	" " "	3·52	5,180	0·72	2·26	4,660	1·01	520	19	501	8	
*‡17	120 " "	3·50	5,215	0·73	2·31	4,690	1·00	525	20	505	8	
*‡18	" " "	3·60	5,110	0·69	2·26	4,620	1·01	490	19	471	8	
*‡19	30 " "	2·48	1,870	0·37	0·89	1,365	0·75	505	3	502	8	Inductive Load on End of Cable.
†20	" " "	2·33	1,933	0·40	0·87	1,332	0·75	601	2	599	8	
*‡21	" " "	2·22	1,852	0·41	0·89	1,398	0·77	454	3	451	8	
*‡22	120 " "	2·16	1,798	0·41	0·88	1,387	0·78	411	3	408	8	
†23	" " "	2·21	1,902	0·42	0·86	1,332	0·77	570	3	567	8	
*‡24	" " "	2·36	1,798	0·37	0·89	1,365	0·75	433	3	430	8	
25	30 " "	6·78	—	—	7·50	—	—	—	208	—	8	Load on Cable.
26	" " "	9·48	—	—	10·00	—	—	—	370	—	8	
27	120 " "	8·81	—	—	9·41	—	—	—	328	—	8	

NOTES:—* Swinburne Wattmeter (*a*) with Potentiometer. † Swinburne Wattmeter (*b*) with 20,000 Resistance direct on 2,000 volts.
‡ Thomson Wattmeter No. 24,125 at far end with Potentiometer.

Mr. Minshall.

Experiment No.	Difference between current put in and useful current leaving.	Experiment No.	Difference between current put in and useful current leaving.
15	1·44	21	1·33
16	1·26	22	1·28
17	1·19	23	1·35
18	1·34	24	1·47
19	1·59	25	– 0·72
20	1·46	26	– 0·52
		27	– 0·60

TABLE III. (*Minshall.*)

Machine Running.	Voltage on Cable.	Exciting Current for Constant Voltage reduced	
		from	to
30 kw. Alternator	10,000	5·8 amperes	2·5 amperes
" " "	5,000	5·8 "	4·7 "
" " "	2,000	5·8 "	5·4 "
120 " "	10,000	17·5 "	13·2 "
" " "	5,000	17·5 "	16·0 "
" " "	2,000	17·5 "	17·3 "

Mr. Fricker.

Mr. G. C. Fricker (*communicated*): The very animated discussion on Mr. Mordey's paper is the best testimonial to its value. Whether the results obtained by him as to the losses in the dielectric of cables are right or wrong, he has certainly established the fact that there *is* a loss well worthy of study, and has brought home that fact in a direct and practical way which practical men will appreciate. I am not in a position to contribute the results of any tests of my own on this point, and indeed the results obtained by the various experimenters which have been already brought before the Institution are sufficiently conflicting to justify considerable scepticism as to the utility of the methods or instruments employed. I would like, however, to suggest that useful information is likely to be got on the subject by making tests at various frequencies and carrying these up as high as possible.

Mr. Fricker. From the known properties of dielectrics in delaying the discharge of a condenser, it is obvious that the power-factor must increase, and, I should imagine, increase rapidly with the frequency of the alternator.

The condenser may be considered as having a periodic time of its own, and just as a frictionless pendulum could swing in its own periodic time without the expenditure of power, but would require energy at any accelerated rate, so must work be done on the dielectric in order forcibly to discharge it synchronously with the periodic time of the alternator. I should therefore expect to find the power-factor for high frequencies go up to a figure even considerably higher than that given by Mr. Mordey, and I think that by carrying out such experiments the exact characteristic curve of the discharge could be found and data deduced which would materially assist us in the right choice of a frequency.

On the latter part of Mr. Mordey's paper I have only congratulation to offer, because I think his device, although doubtless perfectly well understood in principle beforehand, is worked out in quite a unique way, and the intentional and intelligent balancing of capacity and inductance as carried out here is a very different thing from the general sort of feeling of satisfaction a station engineer may have because he finds his transformers help to keep down "surging" in his mains, or the capacity of his mains helps to diminish his magnetising current.

Mr. Sayers. Mr. H. M. SAYERS (*communicated*): Mr. Mordey merits the thanks of electrical engineers for bringing before the Institution some important aspects of alternate current working not hitherto adequately dealt with from a practical point of view. Whether he did or did not measure correctly the dielectric losses in the County Company's cable, his statements have stirred up a useful discussion, and brought to our notice what are virtually to the average engineer "new facts." The difficulty in accurately measuring loads of very low power-factor; the influence of wave form on capacity-current (which I suggest should be called "displacement" current—the "capacity" of a cable is so generally used as meaning current-carrying capacity, that a new word seems needed) are among these. He has also made known an engineering application on a large scale of magnetising or lagging current to neutralise a leading or displacement current. These results deserve the thanks of working engineers at least, and it is difficult to see in the paper any adequate excuse for the aggressive tone adopted towards him by some of his critics.

Choker compensation for displacement current will find practical employment in cable testing, especially in testing cables at high pressures after laying them. In illustration, I may mention that I have in use for this purpose a motor-generator set, the alternator armature of which has to carry a current of 3 amperes, at 2,000 volts alternating. It is thus a 6 K.W. machine working on a maximum load of perhaps 100 watts. This seriously impairs its portability, and made its cost relatively heavy. To test a few miles of concentric trunk main at 10,000 volts has involved the use of large alternators and transformers; and resort has been necessary to such devices as cutting down the frequency and other makeshifts. Adjustable chokers should in future largely reduce these inconveniences. Though ironless

chokers will give closest compensation, they are not readily adjustable, Mr. Sayers.
and probably iron circuits, with variable gaps, will be found most convenient. I have not met with any alternating distributing system in which the leading current preponderates even at lightest loads. The improvement in transformer no-load losses (in which Mr. Mordey has taken a good part), and the rapid displacement of the small "house" transformer, have tended in this direction, but evidently Mr. Mordey has found one case which may be typical. I suggest, however, that excess in this direction should be guarded against by specifying for maxima displacement currents in the cables under definite conditions of frequency and voltage—a course I have taken in several cases. The specification of wave form to be used in such tests seems hardly necessary, as any probable departure from a sinusoidal curve can only increase the displacement current. I have met with serious trouble in mains, apparently due to resonance. Repeated instances of extensive cable break-downs occurred, each case marked by many faults appearing simultaneously. On each occasion these took place during the daytime with light load. As the full load voltage was quite 10 per cent. higher than that during daylight, this seemed strange. But the full load frequency was about 90 $\sim$, whilst during the daytime it was often 70, or even lower. Direct measurement of the capacity of scores of miles of mains connected to hundreds of transformers was, of course, impossible; but calculations based on probable values showed that resonance conditions were quite probable at the lower frequency. Also voltmeter records showed a distinct upward kick just before one of the failures. I, therefore, increased the low-load frequency, and no more wholesale failures occurred. The cables in this case were mostly single rubber, not sheathed, lying in rather large pipes, and their capacity would vary materially with different conditions of wetness in the pipes. Hence the trouble was not constant.

It will probably be found that resonance conditions are likely to be approached in practice as frequency is reduced, and the point must be considered in designing large distributing systems at high pressures.

There is one source of dielectric loss in cables under alternating pressures which has not been mentioned clearly in the discussion, but should be considered. It is due to variable specific inductive capacity in successive layers of the dielectric. Neglecting the ohmic resistance —the slope of potential across each layer is inversely proportional to its specific capacity. If this quantity and the dielectric strength are both low (as in a gas for example), sparking may occur across such a layer. Hence air spaces may increase the dielectric loss, and even where sparking does not occur some molecular disturbance of a lower order, a kind of electrolysis, perhaps, may take place. This may lead to slow deterioration of the material. The porosity of rubber, and the unfortunate experiences in some places with rubber cables, may be due to one or both of the causes above suggested. The heating in German multiphase cables, reported by Mr. Kapp, may possibly be accounted for by their construction. The three-core twisted form with fibrous packing may well have air layers between the individual cores.

When single rubber cables are used as 2,000 volt alternate-current

Mr. Sayers. leads, minute sparks pass between their exteriors when brought into light contact. For this reason the Board of Trade has required such cables to be lapped with metallic foil where passing through boxes and transformer pits, to prevent ignition of escaped gas. With 4,000 volts the sparks are both visible and audible. This air-gap sparking will cause faults in rubber cables in a comparatively short time, and when taking "inner" conductors of concentric rubber mains (the "outers" being earthed) to switch or transformer terminals, I have found it imperative to keep the rubber well away from earthed metal, or else to wrap it closely with earthed foil or strip. The close contact apparently affords sufficient paths for the current to prevent the sparking across air-gaps.

These facts show that disruptive breakdown of insulating layers of low specific capacity occurs under practical conditions even with only 2,000 volts alternating. The losses due to them are of the nature of leakage, but would not be apparent under continuous pressure tests as usually made. Such effects may account for some of the apparently large variations in cable dielectric power-factors. The behaviour of glass, mentioned by Mr. Swinburne, may be due to electrostatic heterogeneity, that substance being a mixture of divers silicates.

I would suggest that the dielectric power-factor may prove to be a valuable indicator of the probable durability of cable dielectrics for alternating pressures, and that an interesting question bearing on this and well worth investigation, is whether the power-factor varies with different pressures. If in any particular material it increases faster than the pressure, this may well be due to heterogeneity and an indicator of rapid deterioration. Our present information as to dielectric permanency under alternating pressures is confined to a very few materials suitable for cable insulation, has been purchased at a high price, and is only capable of extension by costly and lengthy experience unless some such test as that now suggested should be found to give reliable guidance.

In respect to the measurement of these very small power-factors, it would seem that an electrostatic wattmeter could be devised free from the sources of error which are inseparable from wattmeters of the dynamometer type—since every current producing a field must have some lag. Such an instrument would meet a want.

In paper cables I have found a somewhat marked discrepancy between the capacity tested by the usual ballistic method and the results of displacement currents at high voltages and frequencies. Rubber cables have shown a much closer agreement. My opportunities of comparison have, however, been few, and insufficient to serve as a basis for a general statement. The tests were made with alternators, giving nearly sine curves. The much larger displacement currents due to E.M.F. curves roughened by strong harmonics, or sharply peaked, are obviously due to the real frequency being higher than that of the fundamental in the one case, and to the maximum P.D. being higher in terms of the virtual P.D. in the other case.

This difference is favourable to alternators giving sine curves, the virtues of which seem to be rather better appreciated now than they were a few years since, when it was fashionable to belaud iron armature

alternators, despite their irregular and steeple-like E.M.F. curves, poor characteristics, and rather low efficiency. That they could be "short-circuited with impunity" appeared then to be the only virtue that counted. Mr. Sayers

On the important question of the cost of producing the displacement current, my experience is that a generating set cannot be run at full voltage for less than about 20 per cent. of the cost of running it fully loaded. It follows that "phantom" watts are relatively expensive, whether due to lagging or leading currents. The former are worse because greater excitation is needed for a lagging than for equal leading current. Hence any practicable device for reducing the "idle current," whether leading or lagging, may have a considerable value to alternating supply systems. The condenser has been long ago suggested to cancel the lagging current, but a commercial form does not seem to arrive. The choker for the leading current may have a better chance. It is not to the point that one adds choker losses to condenser losses; if these two can be supplied by running a small unit instead of a large one, the saving in cost of running will quite outweigh the little extra energy wasted outside the alternator, and even the total energy wasted will probably be much smaller.

Even in the extreme cases cited by Mr. Mordey, the dielectric losses are not a large fraction of the total energy transmitted by the supposed cables, if their load-factor is a reasonably good one, and it is certain that good load-factors are indispensable to the commercial success of long-distance transmission undertakings under usual conditions. Where this is not the case (as in some of Professor Forbes' suggested instances) transmission losses will be of trifling importance.

An instrument, fit for a central station switchboard, which would show phase difference and its sign, at any moment, for the whole load of the station, any feeder or alternator would fill an empty place very acceptably. Professors and instrument makers have here an opportunity of added usefulness. The oscillograph is hardly the tool for such work obviously, though its power to investigate some of the problems of alternate-current work would seem to be great, and useful results should soon make themselves evident.

Mr. Stuart A. Russell (*communicated*): Mr. Mordey's paper may be divided into two parts—first, that which deals with the wattless condenser current, and the means by which this current can be provided without calling on the generator to supply it; and secondly, that which deals with the hysteresis loss in the dielectric. With regard to the first we have been told in the discussion that the paper contains nothing new or which was not known to many people ten years ago. This may be so; but those who knew do not appear to have been able to put before the men practically engaged in the generation and distribution of alternating currents a clear statement of how to provide the condenser current for their cables without supplying that same current from the generator. After the last meeting I tried to get a copy of the Physical Society paper referred to by Professor Ayrton, but was unable to do so, as it does not appear to have been published in the Proceedings of the Physical Society. I have therefore had to fall back on Mr. A. Russell.

Mr. A. Russell.

the abstract published in the technical journals, where I find a bald statement that "On joining a condenser and inductive coil in parallel, an ammeter in the main circuit indicated 5·5 amperes, whilst those in the branches showed 6·4 amperes passing through the condenser and 10 amperes through the coil." This result compares very unfavourably with that which Mr. Mordey has obtained, and does not, to my mind, suggest that the authors of the paper had any idea of the practical application to which Mr. Mordey directs our attention, or they would have given an example showing a far greater difference between the condenser current and the main current. Again, in the discussion on Dr. Fleming's paper, read in 1891 before this Institution, most of the people spoke who might be expected to know about this matter; but I find that not one of them, not even Professor Ayrton, drew attention to the fact that there was a means of providing this condenser current, amounting to about 45 amperes with 10,000 volts or 600 apparent E.H.P. without calling on the alternator to produce it. It appears to me, therefore, that Mr. Mordey's paper should be cordially welcomed, at any rate by everybody engaged practically in the distribution of high pressure alternating currents, as it directs attention, in what he calls the vulgar tongue, to the losses incurred by supplying this condenser current from the main generators, and further tells us how these losses may be reduced to a minimum.

With regard to the hysteresis loss in the dielectric, there appears to be little doubt but that Mr. Mordey has exaggerated this loss; but his paper has stirred up interest in the question to such an extent that our real knowledge on the subject is likely to be much increased by the experimental data which are now being collected by various people. These experimental data are much wanted, as the power-factor has been too much spoken of, both in the paper and discussion, as though it were a constant, and there is much to be learnt concerning the effect of variation of stress in volts per centimetre, of the imperfect homogeneity of the various dielectrics in practical use, and of the varying conditions of manufacture and of ageing of the dielectric, all of which will, I think, be found to affect the result considerably.

Mr. Esson.

Mr. W. B. Esson (*communicated*): I am not sure that I agree with Mr. Mordey as regards what he calls the "Ohm's law" of the subject, without further investigation. Certainly the readings he obtained and which he plots in Fig. 1, if they can be held to prove anything at all, prove that the current rises in a *curve* and not in a straight line, and proportionality to the pressure is not manifested, as has been assumed by the drawing of the line with a ruler. There are good reasons for believing that the current is not proportional to the pressure, and, in the absence of very complete and lengthy investigation, it would be unwise to assume about this matter an Ohm's law or anything else. The capacity of insulating materials under different stresses and under long and continuous stress is very imperfectly understood.

The action of a choking coil in supplying capacity-current has, of course, been long known, though I am not aware that it has ever been proposed before to employ it as part of an electric station equipment. Years ago the matter was fully understood, and its not being adopted

in the above sense is probably because the composition of the circuits in a general scheme of distribution is such as always to cause the currents to lag, capacity consequently becoming an actual advantage. With the longer lines of the Power Bill schemes there will of course be increased capacity; but from their nature the power circuits again are more likely to tend to a lag of current, and here capacity may prove, as before, a blessing. In power schemes with overhead lines the equivalent of capacity is sometimes purposely introduced, and I have known one at least where the capacity of the five-mile underground line proved salvation, as certainly but for this it could not have worked, *i.e.*, unless some form of phase rectifier had been added. Mr. Esson.

The thanks of the Institution are due to Mr. Mordey for having brought prominently to the attention of the members the hysteresis loss in cables, and his paper will accordingly have good results. But of Mr. Mordey's figures for these losses the less said the better. The author does not appear to be lucky as an original investigator, and it is perfectly clear that, whatever in his experiments on the County cables he was measuring, it was not power, and that the results he obtained are something like four times larger than they should be. This is important because it is the power that costs the money. The wattless or capacity-current, contrary to Mr. Mordey's views, costs nothing unless the capacity is so abnormally great as to necessitate sensibly larger engines being coupled to the generators than would otherwise be required. This is an extremely unlikely case; but, if it were so, the only extra cost would be in the extra steam required to drive the larger engines idle. Say, on account of capacity, we had to put in a 1,100 k.w. set instead of a 1,000 k.w. set. Working condensing we should use an extra 500 lbs. of steam per hour right through, costing perhaps 5d. Such a set would never be run at less than half load, so it is easy to see that the addition to the cost of the unit is quite infinitesimal.

Professor E. Wilson: Assuming a power-factor of ·124, Mr. Mordey calculates that at 6,000 volts and 50 periods per second this cable dissipates 1,206 watts per mile. With 47·2 amperes in each conductor the copper loss is also 1,206 watts per mile. The total dissipation in the $5\frac{1}{2}$ miles of this cable is therefore 13,300 watts, and this is 4·7 per cent. of the power which would be transmitted at unit power-factor, namely 283,000 watts, or it is 9·4 per cent. with a power-factor 0·5. If this dielectric loss is a fair representation of average practice, I think one might apply a differential test so as to measure it as a direct quantity. Professor Wilson.

If V and A be volts and amperes at transmitting end and $V-v$ $A-a$ be volts and amperes at receiving end, then $Av+Va-av$ is the loss in the cable at any moment, and can be split up into two classes of loss, namely, $v\left(A-\frac{a}{2}\right)$ and $a\left(V-\frac{v}{2}\right)$. If this expression be integrated over a half period and divided by the time of a half period, the result is the average watts required to be measured as a direct quantity. Two wattmeters having no mutual induction, but having their moving systems rigidly fixed together, might be employed. Each wattmeter would be working on a large power-factor, and the loss would be

Professor Wilson.

measured when the cable is loaded, and when the full self-induction of the cable is felt. The dissipation of energy in the dielectric may be the same when the cable is loaded and unloaded. This is, so far as I know, a still unsettled point in connection with alternate current transformers, in which it has been stated that the core-losses are smaller on load than on open circuit.

I have looked carefully and cannot find the thickness of rubber in this cable, or any means for calculating the specific-inductive capacity. This is an important matter, as we ought to know the volts per cm. of dielectric thickness.

I think it would greatly aid in drawing further conclusions if Mr. Mordey would also give particulars of the wave-form and the way in which the instruments were tested for accuracy.

Mr. A. Russell.

Mr. ALEXANDER RUSSELL (*communicated*): I have been very interested in studying Mr. Mordey's results. His paper has certainly given us clearer notions of the difficulties due to capacity which alternating-current engineers have to overcome. We have not yet sufficient experimental evidence to justify us in adopting any simple empirical formula for the dielectric loss. It would be very wonderful if it were simply a function of the effective voltage and independent of wave shape or area. Whatever is the real action that goes on in the dielectric, I think that we are justified in concluding with Mr. Steinmetz that it distorts the wave of P.D. This distortion may be small; but even a small distortion considerably modifies the balancing action of condenser and choking coil currents. For example, a parabola is practically indistinguishable from a sine curve—it has the same rounded shape and is only about three per cent. flatter; yet if the wave of P.D. at the terminals of the cable in Mr. Mordey's experiment (Fig. 4) were parabolic, then it would be impossible to cut down the current in the main to less than 0·65 of an ampere, even if there were zero loss in both the cable and the choking-coil. If the wave were triangular, then the minimum current would be 2·45 amperes, and if the wave form were rippled or very peaky, the balancing action of the choking-coil would be of little use.

It is important to realise the difference between the action of condensers and choking-coils on the harmonics in the applied wave. The condenser current shows the harmonics magnified, but in the choking coil-current they are to a certain extent smoothed out. The current-wave in a condenser is less like a sine curve, but in a choking coil it is more like a sine curve than the wave of P.D. at the terminals. It is this action of condensers and choking-coils that makes the sine form of wave produce in them the minimum and maximum currents respectively. It also has a bearing on the "resonance" method of testing used by Mr. Mather. In this method the same current flows in both the choking-coil and the condenser. Hence the wave of P.D. across the choking-coil terminals is more distorted than the wave across the condenser terminals. It follows also if the circuit be adjusted for resonance of the fundamental harmonic that the P.D. across the choking-coil terminals will be greater than the P.D. across the condenser terminals.

I should like to point out the limitations to the use of the ordinary

Mr. A. Russell.

formulæ for condenser and choking-coil currents. The formula for the condenser current is

$$C_1 = \alpha V K f$$

where α is a constant depending on the shape of the wave. The smallest possible value that α can have is 2π, and it has this value for the sine curve. For a parabolic wave the error in taking 2π for α is 0·7 per cent., and for a triangular wave it is 10 per cent. If it is rippled or very peaky the error may be much greater.

The formula for a choking-coil is

$$C_2 = \frac{V}{\beta L f}$$

where β is a constant depending on the shape of the wave. The minimum value of β is 2π, but the error made in taking 2π for β is much smaller than in taking 2π for α. Even for a triangular wave it is less than 1 per cent. This is due, of course, to the constraining action of the choking-coil forcing the current wave to assume an approximate sine form. The graphical phase difference ϕ between C_1 and C_2 is given by the formula

$$\text{Cos}\,\phi = -\frac{\beta}{\alpha}$$

Now, for this phase difference the minimum current in the main got by varying C_2 is $C_1 \sin\phi$; for very peaky waves ϕ is nearly 90 degrees.

The values of C_1 and C_2 do not change much for a small distortion of the wave from the sine curve form, but the resultant current changes rapidly, hence a measurement of this current might be used to test how nearly the P.D. wave produced by an alternator approached the sine form.

In Mr. Mordey's experiment (Fig. 4) the condenser current vector makes an angle of 82° 53′ with the P.D. vector, and the choking-coil current vector makes an angle of 87° 39′ with the same vector. The sum of these two angles is 170° 32′, but the angle between the current vectors is only 164° 26′. Hence we must represent the three vectors by lines drawn in space. The vector representing the current in the main is always in the same plane as the other two current vectors, as a linear relation connects the instantaneous values of the three currents. Hence the mimimum phase difference between the main current and the P.D. vector is the minimum angle between the P.D. vector and the plane containing the currents. Now if α, β and γ be the three angles of a solid angle, then the minimum angle θ between one of the edges and the plane opposite it, which we suppose to contain the angle γ, is determined by the equation

$$\sin\theta = \frac{\sin\omega}{\sin\gamma}$$

where $\sin\omega = 2\sqrt{\sin\sigma\sin(\sigma-\alpha)\sin(\sigma-\beta)\sin(\sigma-\gamma)}$ and σ is half the sum of the angles α, β and γ. Substituting Mr. Mordey's phase differences we find that θ is 52° 24′, and cos θ is 0·61. This is the maximum possible power-factor with the phase differences with which Mr. Mordey was working, and this is the result he obtained.

The maximum power-factor of the combined circuit being only 0.16

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shows that the shape of the resultant current wave is very different from the shape of the P.D. wave. A triangular and a sine curve have a maximum power-factor greater than 0·99.

I agree with Mr. Mordey's remarks about the value of condensers for increasing the power-factor of alternating current networks. I would suggest that, instead of putting the condenser straight between the mains, we put it across the secondary terminals of a step-up transformer, the primary terminals of which are put between the mains. For example, if we put a one microfarad condenser across 500-volt mains, the frequency being 50 we only get a current of about 0·2 of an ampere. If, however, we put it between the 5,000-volt terminals of a step-up transformer, we should get a current of 2 amperes in the secondary, and there would therefore be a current of 20 amperes in the primary with a large angle of lead. If n be the ratio of transformation the current is increased n^2 times. So that quite a small condenser can be used to get large balancing currents.

In Mr. Mordey's choking-coil as the current diminishes so also does the angle of lag. If we wish to vary the lagging current, keeping the angle of lag always very large, we could easily do this by putting choking-coils in parallel across the secondary terminals of a transformer the primary of which was connected between the mains.

Mr. Addenbrooke.

Mr. G. L. ADDENBROOKE (*communicated*): Mr. Mordey's paper is a difficult one to deal with, as it raises at least four important and specific points, each of which has not necessarily much connection with the others. As, however, I have worked over a great deal of the same ground during the last few years, I venture to submit the following remarks on points deduced from my own experiences.

The capacity-current in systems of mains is one I have always had fully in view in dealing with large power systems. Although I agree with the letter from Dr. Hoor in the *Electrician* of February 8, 1901, that the effect is capable of being dealt with, still Mr. Mordey has done good service in drawing attention to the fact that the effect may be large, and I think this is most likely to be the case in the early days of power distribution, when large systems of conductors have had to be laid down and the load is comparatively light. It appears to me that all Mr. Mordey says under this heading is a remarkable commentary on the unwisdom of the Board of Trade Regulations which practically prohibit overhead wires in this country. Although power distribution on an enormous scale has been carried out on the Continent, and in America, it has almost wholly been carried out by means of overhead wires, where the effects described by Mr. Mordey do not occur. The Board of Trade prohibits us adopting methods by which great industries have been safely and advantageously built up on the Continent and in America, and forces us to adopt expensive and untried methods, which at any rate are liable to serious inconveniences even if by skill and care they can be got over.

Coming now to the watt loss in the dielectric, I must say I was very much startled by Mr. Mordey's figures. While aware for many years that there was a loss of this character, I had no idea that its magnitude was at all comparable to the figures given for it. Since Mr. Mordey's

paper I have been too busy to make any direct measurements on a cable itself, but the following results obtained on condensers may be interesting. The measurements were carried out entirely with electrostatic instruments such as described in the paper I read before the International Congress of Electricians at Paris in August last. Without giving a description of the details—I may say that an electrostatic system such as described is particularly suited for this work, and I think that the results can be obtained with a very small percentage of error. Mr. Addenbrooke

Some months since Dr. A. Muirhead was good enough to place at my disposal a set of condensers for experimental purposes—in fact, for investigating amongst other points this very effect. These condensers were connected to an alternating current of 1,000 volts, and about 30 periods, and careful tests which Mr. C. W. S. Crawley was good enough afterwards to confirm with the writer, show that under these circumstances the power-factor is about ·011 to ·012. In fact, deducting a small fraction for loss in leads, about 1 per cent. of apparent watts is really true work. The experiments were made on condensers of three and six microfarads capacity respectively, which were probably equal to about seven to fourteen miles of ordinary cable. It is perfectly true that these condensers were made by Dr. Muirhead with great care in order to have the absorption small; but, on the other hand, it must be remembered that the dielectric is very much thinner than that of an ordinary cable, which probably would also be made of paper, and therefore the potential slope is very much greater than in a cable, which, whether rightly or wrongly, I have always taken as a serious factor.

Coming now to Mr. Mordey's suggestion of the use of a choking-coil across the circuit and the data he gives of a coil he has made. I may say that there has recently been completed to my specification an adjustable inductive resistance having a capacity of 170 k.w. on Mr. Mordey's rating. This inductive resistance is intended for putting a lag in the circuits of alternators under test. It has been wound for practical purposes with a number of circuits which have permitted a somewhat elaborate series of tests to be made on it. It would have been interesting to have compared its performance with that of the resistance made by Mr. Mordey, and I should therefore have been glad if Mr. Mordey had given the current which his apparatus takes with the iron circuit closed as compared with the iron circuit fully open, with a given voltage between the terminals. It is not possible to close the iron circuit of such an apparatus as well as in an ordinary transformer, and therefore it takes a larger wattless current than a transformer with overlapping plates, but how much it is difficult to say without experiment, as calculations are not of much value. In the large inductive resistance I have mentioned, the construction is such as to avoid as far as possible the effects from eddy currents Mr. Mordey speaks of, and though I think the tests show some extra loss from this cause beyond the losses which would ordinarily be allowed for in a simple transformer, yet I do not think it is nearly as large proportionately as the figures Mr. Mordey gives for it. At some later date I may be able to give further data regarding the work on this resistance.

Lastly, I would like to say a word on Mr. Mordey's wattmeter. In

Mr. Addenbrooke. the work I have done during the last five or six years on alternating current measurement and, in fact, since I first took the subject up some years before that, I have on numberless occasions been tempted to make apparatus constructed with transformers for raising the voltage, but I have always been deterred by the uncertainties I expected to be involved in. I think it quite possible that such an instrument, tested and calibrated by instruments which can be relied on, may be useful for approximate measurements through a moderate range and comparatively high power-factors, but I have felt that to go beyond this required great caution. Some experiments I made some months since, using electrostatic instruments, confirm the view Mr. Duddell has recently expressed in the technical press, that there was a certain amount of lag produced by magnetic leakage which at any considerable power-factor would entirely upset readings.

In the course of the debate on Mr. Mordey's paper, Dr. Fleming advocated making tests with a motor transformer and accumulators. As a matter of fact, my own tests were made up in this way, except that instead of accumulators I used the Westminster Co.'s current, the voltage being kept steady by passing the current through a variable resistance and observing a voltmeter. Owing to the leading current produced by the condenser, the reactions in the generator are considerably altered when the condensers are put on, and it is necessary to observe carefully that the speed is similar when any comparative measurements are made.

Mr. Boot. Mr. H. L. P. Boot (*communicated*): In the first place, the author is to be congratulated on presenting to a technical body a paper which is a masterpiece of simplicity, and in endeavouring to explain the subject in (as he terms it) "the vulgar tongue" so satisfactorily. Capacity measurements are taken by some of the station engineers to my knowledge, and no doubt if the results obtained, were anything like the magnitude of the loss the author has obtained they would be taken more frequently; but experience has shown that, in practical everyday working, these losses are negligible, because on nearly all alternating systems, owing to the number of transformers in use, there is bound to be a lagging current, which will more than counterbalance the capacity current. I should very much like to see the results showing the variation of capacity after the life of the cable, and whether the ordinary fibrous or paper insulated cable decreases in capacity, as one would expect it to, as it becomes drier.

On page 377 I cannot help thinking that the author takes rather an "alarmist's" view of the losses due to capacity, as I have been unable to confirm any losses of such a serious amount in concentric cables when insulated with paper, and I fancy a great deal of the loss must have been due to the dielectric employed on this particular cable. If the author would kindly inform us whether the cable he conducted the test on has been in active use? At what pressure? For how long, and in what condition the cable was afterwards, so far as insulation and withstanding the pressure test? I certainly have noticed when switching long feeders on that an engine, especially if it be a small unit, has been checked, but this, I think, has been due to the

work done in charging the cable, which, of course, is more or less a momentary loss, unless for some reason or other the cable becomes discharged by leakage and a continuous charging is taking place. However, if time permits this summer, I intend to make a careful test on a feeder three miles long for at least a period of three hours, measuring carefully the steam consumption of the engine running light, without the cable being switched on, and afterwards the steam consumption of the engine with the cable switched on; in other words, the engine having only to maintain the pressure. It is then clear that the difference in steam consumption will show the actual amount of work done, which I expect to be very little. Mr. Boot.

For some time I have been endeavouring to get at the true cost of apparent watts, so far as the actual coal consumption is concerned, or the work done by the engine, and I agree with the author in thinking that it is not much *less* than one-fourth. In practical working one would not, of course, keep a feeder charged without being connected to a transformer, and doing work of some sort, and therefore the conditions under which the author made his test are not applicable to practical working, although extremely interesting and instructive.

Some five years ago, when the Tunbridge Wells works had only been running a year, and the total amount of cables laid would not exceed ten miles, the apparent watts, *i.e.*, amperes multiplied by the volts, were considerably in excess of the true watts registered by the watt-hour meter, whereas now we find, although using the same instruments as previously, that the true watts now equal the apparent watts, *i.e.*, the reading of the watt-hour meter is nearly the same as the reading obtained when multiplying the amperes by the volts (multiplied by the time). It is possible this may be accounted for by having now some forty miles of concentric cables laid (both high and low pressure), which should have a large capacity effect and assist the lagging current, so as to bring the power-factor as near unity as possible. There is no doubt that a certain amount of work is done when keeping a cable charged, as one can usually tell whether a cable is alive or dead by grasping it tightly over the insulation, when, if alive, a distinct vibration will be felt.

Mr. G. H. NISBETT (*communicated*): Mr. Mordey has rendered a considerable service to the Institution in bringing the subject-matter of his paper to the attention of the members. It covers ground that undoubtedly has been very neglected in the past, and, whether Mr. Mordey's figures are right or wrong, the paper will have done good in inducing other people to make experiments on the lines of his. Mr. Nisbett.

The first part of Mr. Mordey's paper appears to be somewhat elementary, and I think he hardly gives central station engineers credit for the knowledge that is in them.

The feature of the paper is the reference to the losses due to dielectric hysteresis, or the power-factor of the capacity-current of a cable. The losses shown are so considerable, and so much in excess of those that it has hitherto been assumed are taking place, that a considerable flutter in the alternating dovecotes has occurred.

To show how serious a matter it is one need only instance the case

Mr. Nisbett. of the Deptford Station, where (if Mr. Mordey's statements are correct) 530,000 units per annum are wasted in dielectric hysteresis losses; and Mr. Mordey apparently wishes us to believe that the station engineers have never missed this energy! Luckily for Deptford and other extra high tension stations, there appears to be no reasonable doubt but that Mr. Mordey's figures are very seriously in error.

In the first place, I think it will be generally agreed that Mr. Mordey's use of a Thomson integrating wattmeter for the measurement of energy having so low a power-factor was a mistake. When such a meter is used to read capacity-current or nearly wattless voltamperes, if there is the slightest self-induction in the shunt coil the whole of the readings will be completely upset. (The use of a transformer in the shunt would make things infinitely worse.) One can see this when it is considered that, assuming a 2½ per cent. power-factor, the angle of the current we wish to measure is 88¼ degrees from the volts, an absolutely wattless current being of course 90 degrees. To show how difficult it is to measure currents of such low power-factors by ordinary methods, I may say that after the reading of Mr. Mordey's paper I made several experiments with a view to determining the power-factor of a cable. In the first place, by the three voltmeter method. This was a complete failure, probably owing to the fact that the errors in the voltmeters are squared, the results showing a small negative power-factor, *i.e.*, that the cable was giving energy to the circuit. In the second case I took a Thomson watt-hour meter of the type used by Mr. Mordey, but without a transformer, and put on to it a non-inductive load of about one ampere. The speed of the meter was then measured and then a capacity current of one ampere added to the circuit. There was no observable difference in the speed of the meter, thus showing a zero power-factor. The third method was with a Weston wattmeter, the shunt being connected across a known proportion of a non-inductive resistance. This method showed a power-factor on paper cable of ·0125, or about half what I believe to be the true result, the error no doubt being due to the self-induction of the fine wire coil.

I have had the pleasure of witnessing tests carried out at the works of the British Insulated Wire Company by Professor Ayrton, and referred to in Mr. Mather's remarks. The results attained by the three methods used checked so exactly with each other and with others made by different sets of instruments, that I am convinced the power-factor of ·024 for paper cable given by Mr. Mather is the correct one. This is confirmed by figures given by Dr. Hoor in the *Electrician*, in which he gives for the Buda Pesth paper cables at 2,000 volts a power-factor of ·025.

The alarming figures shown in Mr. Mordey's tables are based not only on too high a power-factor but on too high a capacity, for the cable tested by him was a rubber insulated one, whereas this type of cable is comparatively seldom used for high tension work. By far the commoner type is paper insulated cable, and this has a capacity of about 0·4 that of indiarubber, and a special dry core high voltage cable made by the British Insulated Wire Company has a capacity of

only one-third of that of indiarubber. Therefore, to arrive at an approximation of the energy loss of cables as commonly used we must divide the figures given by Mr. Mordey by about 15. Mr. Nisbett.

To show what the loss due to dielectric hysteresis will amount to in practice, I have worked out two concrete cases.

In the first case, that of the Midland Power Company, who are laying down a system of 7,500 volt two-phase cables over a very extensive area, some of the feeders being eight miles in length. The total apparent power in the charging current of the cable will be 290 k.w., and the true energy loss in the cables 7·25 k.w. The station will have immediately 5,000 k.w. of plant, and assuming a 12½ per cent. load-factor the loss in dielectric hysteresis will be 1·2 per cent. of the units sold. When the mains become loaded to their full capacity the loss will then be only 0·4 per cent. of the units sold.

In the second case I have taken as an example a large town now working with a monophase system at 2,000 volts, and supplying over 100,000 lamps. The total capacity of the high tension network is 10 microfarads. The dielectric hysteresis loss is 520 watts, or 4,500 units per annum, and, the output of the station being 2,000,000 units per annum, the energy loss by dielectric hysteresis amounts only to something less than one-quarter per cent. of the energy sold. I think these figures go to show that the losses due to this cause are really insignificant.

Mr. Mordey suggests that dielectric hysteresis differs from magnetic hysteresis in showing no saturation effect, while Dr. Hoor holds that the power-factor diminishes with increased pressure. I am inclined to think from observed facts that the contrary is the case, and that power-factor increases with increased pressure. I am led to this opinion by some experiments made on a short length of cable built for a working pressure of 10,000 volts, and tested to 90,000 volts 100 frequency. After the application of this pressure for half an hour, although the cable showed no signs of breakdown, it was distinctly hot to the hand. This heating could not be due to conductance of current through the dielectric, as of course the insulation resistance of so short a length would be many thousands of megohms, but could only be due to dielectric hysteresis of a very much higher order relative to the capacity-current than even Mr. Mordey's figures show.

With regard to Mr. Mordey's suggestion of counteracting capacity current in cables by putting a coil with self-induction in parallel, I think Mr. Mordey certainly deserves the credit in this matter of having proposed to apply known principles to practical purposes in a systematic way, although of course the principle underlying his suggestion has been well known for many years.

I remember in the very early days of the City Company that we got into trouble by using single cables for alternating currents, and we seriously discussed the feasibility of putting condensers on the mains to neutralise the self-induction, but on calculating it out we found it cheaper to use triple concentric cables. This, of course, is exactly the converse of Mr. Mordey's suggestion. It is to be remembered that the capacity-currents of central station cables are more or less neutralised

Mr. Nisbett. by the transformers connected to them, and it is rare to find a station working at 2,000 volts in which the wattless current due to the cables exceeds one or two amperes. Certainly not a noticeable amount even at the lightest loads.

With regard to extra high tension stations, the annulment of the capacity may be of use, although its application presents difficulties. In the first place, it must be remembered that the use of the self-induction coil will add to and not diminish the total energy losses of the station, and that it is only of use at all on very light loads, for as soon as the real working load comes on to the station the capacity current practically disappears. This, of course, is due to the condensers and wattful currents being in quadrature; the condenser current is represented by the square root of the difference of the squares of the current at the source and end of the cable. This means that, if the capacity-current in the cables at a certain station is 6 amperes, then with 10 amperes of real work on the circuit the total current is represented by 11·7 amperes. With 50 amperes real work on the station the total current is 50·6 amperes, and with 100 amperes of real work 100·2, so that in this last case only ·2 amperes would remain for Mr. Mordey to neutralise, and the energy loss in his choker would probably amount to more than this.

It is therefore evident that if the device is to be of real service at the station it must only be used on very light loads, and must be switched off as soon as the work becomes appreciable.

Unfortunately, Mr. Mordey's device will not prevent undue momentary rises of pressure in high tension cable systems having capacity due to any disruptive discharge that may occur, as owing to the enormous frequency of such discharges the coil would be practically infinitely resistant as a path for current.

The device will probably prove of considerable advantage to cable makers in testing batches of cable with high pressure alternating current, as a suitable adjustable choker will enable the total current given by the alternator to be considerably reduced, and so allow the use of smaller alternators than would otherwise be possible.

I should like to know where the demand for condensers Mr. Mordey speaks about is to be found, as it can readily be met. A good condenser in the form of a cable having small conductors of large surface, and with a dielectric having a high specific capacity, can be built for a working pressure of 2,000 volts, at about £30 per microfarad.

Mr. Cruise. Mr. E. G. Cruise (*communicated*): The salient points of Mr. Mordey's most interesting paper were, at least as I understood them, that first, electrical transmission of large powers at high voltages over long lengths of underground insulated cables was going to be at least enormously handicapped if not defeated altogether, due to the capacity losses in these insulated cables; and second, that it was very desirable for electrical engineers seriously to consider the question of how the problems involved are to be dealt with. All this in view of the recent legislation and passing of the Electric Power Bills of 1900 and our entry in this country on a comparatively unexplored and untried

electrical engineering venture. Except from Mr. Mordey himself, we failed to hear any definite suggestion as a remedy for the evils in question. Mr. Cruise.

If I presume to join in the discussion it will be to try and analyse at least one of the practical points raised by Mr. Mordey, *i.e.*, the extent of the losses. I would first like to ask Mr. Mordey a question, and it seems an important one. What was the length of cable referred to in Table I., and what was its ampere carrying power? It had a capacity of one microfarad, and the watts lost due to capacity under different voltages are given.

In the Electric Power Acts now about to be carried out, the question of k.w. transmitted in a cable, compared with the accompanying capacity losses per mile, seems to be one of cardinal importance where long distances and large powers are to be dealt with, and I want to try and show from some figures that one microfarad capacity apparently goes a long way in a high tension power transmission cable. We may arrive at the end in view from perhaps the following consideration. First of all assume that the capacity power-factor, ·124, found by Mr. Mordey is neither too high nor too low, and take it as a basis. If it can be proved to be excessive, so much the better for the arguments. Judging from the discussion at the Institution, it is not likely to be proved insufficient. Now, Mr. Mordey calculates in his paper that, with 40 miles of "go and return" concentric cable of ·5 M.F. capacity per mile running at 50 periods and 20,000 volts, the annual capacity loss would be about 2,700,000 Board of Trade units.

Assume that the cable contemplated was a 37/16 and that ·5 m.f. per mile for this size is practical. Such a cable could easily take 2,500 k.w., ten miles at 20,000 volts. Suppose in some power scheme that there were four such transmissions in various directions feeding different centres, and thus making up the 40 miles of cable.

If 5 per cent. is allowed for other losses in the cables, apart from capacity losses, it is practical to suppose that this network of cables would be dealing with some 10,500 k.w. at their outgoing ends. Now, it was satisfactorily demonstrated during the parliamentary proceedings of the Power Bills, that in a comprehensive electric power supply, a load factor. of 25 to 30 per cent. was to be reasonably expected, if it did not exceed these figures. Take 10,500 k.w. on, say, a 30 per cent. load factor and we get

Board of Trade units per annum	= 27,405,000
Board of Trade units lost in capacity effects ditto	= 2,700,000
Total	30,105,000

Thus the ratio

$$\frac{\text{Units generated per annum}}{\text{Units lost in capacity effects}} = \frac{30{\cdot}1}{2{\cdot}7} = 11 \text{ (approx).}$$

From this results that the units lost in capacity taken over a year would be some 9 per cent. of the total units generated for the supply. This figure of 9 per cent. might be further reduced by reducing the periodicity, say, to 40, the capacity to ·4 M.F. per mile or less, and so on.

Mr. Cruise On the other hand, it would be increased by diminishing the load-factor. Of course it must be admitted that this 9 per cent. added to the other transmission losses, makes the total loss in the H.T. mains, even with a load-factor of 30 per cent., rather formidable, though I can scarcely think it would necessarily quite spoil a dividend on the capital expenditure. That could be easily gone into, though I do not propose to do so here. But it must also be remembered that this 9 per cent. is arrived at assuming generally unfavourable conditions; for example, no compensation for capacity-currents through induction, the periodicity and voltage unnecessarily high for the conditions named, and so forth.

Mr. Mordey shows that dielectric hysteresis is the only important one of the capacity losses in the mains for large cables. He also says this loss is not preventible and cannot be counterbalanced like the capacity-current itself, but that this hysteresis may be lessened.

In this connection I would submit two points; first, that if it is really of the nature of a mechanical strain in the dielectric that strain will be diminished when the charging currents and charges helping to produce it are compensated, even though the periodicity remain the same; and second, that in the H.T. cables likely to be used in the power schemes the dielectric will be thick and so, as in Mr. Mordey's opinion, the strains will be lessened.

I mention these two points, though one of them is but a conjecture, by way of emphasising the fact that when Mr. Mordey's factor ·124 is taken, as is done here, in any calculations, we must be well on the safe side. But, as already stated, 9 per cent. annual loss in capacity alone, while not quite a desperate one, is a very undesirable one, and all means of diminishing it should be investigated by electrical engineers. That capacity, on the other hand, under certain conditions produces most favourable effects, where it is present as a set-off against self-induction, is undoubted, and for any given cases the figures can be calculated.

Mr. Mordey proposes to vary the self-induction and balance with chokers. Some continental engineers have contemplated varying the capacity, always an increase of course for a given cable, but so far this latter course has not had practical success.

In Mr. Mordey's method the difficulty would seem to be to have practical means of controlling these chokers if they are placed along the cables themselves instead of at the generators. For the choker current at the plant full load and light load would have to be very different, and, in fact, the range of "apparent watts" in the chokers, over comparatively small changes of plant load, would be probably enormous. I would like to have Mr. Mordey's opinion on this point, because, while it scarcely applies to Mr. Mordey's case, in dealing with this subject in Germany recently, it has been urged that the suitable distribution of the chokers along the transmission cables is the best arrangement rather than having them all at the generating source. For in the latter case the alternators alone may be protected from the economical disadvantages of large wattless currents at light loads, while in the former case the mains themselves may also have their otherwise present charging currents compensated in part or

Mr. Cruise.

entirely, and so, as I take it, the cable heating and hysteresis losses probably sensibly diminished.

I wish to add a few words about capacity effects in 3-phase 3-core cables, as I have been going into the subject recently in connection with one of the Electric Power Acts. On first sight it might seem a complicated subject, but on investigation I think it proves not to be so. At least, I refer to the same nature of effects and losses dealt with in the paper under discussion. Mr. Mordey says he has not in this paper compared the question of capacity of 3-phase cables.

It seems to me that as these are the very form of cables likely to be largely used, for other economic reasons, in the power schemes, that the consideration of them in brief is an interesting subject, and, as I hope to show, has a direct and important bearing on the present paper and discussion.

I have been able to get some figures together which may be of use to the members, and would like to have Mr. Mordey's opinion on them. The important point is that, if they are correct, the capacity losses are going to be notably less in three-phase, three-core cables than the losses found by Mr. Mordey in single-phase concentric cables. I am entirely indebted to Monsieur C. H. Guye, an eminent continental electrical expert, for many of the statistics given in working out these calculations, and he has kindly given me his permission to make use of them here. Some of the actual measures of capacity have already appeared in the continental technical papers, but I have extended them and reduced them to British units.

In the adjoined table there are two different examples taken, one for 10,000 volts and one for 20,000 volts. These are shown respectively as I. and II. The cables are lead-covered and armoured. They have been recently constructed by a large continental cable firm.

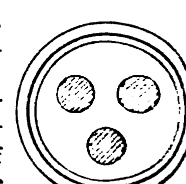
Mr. Cruise.

Capacity Effects.

Three-phase, three-core cables. "Star"-connected. Neutral points to Earth.

		Case I.	Case II.
		Insulation :— Conductor to Sheath $= \cdot36''$ Between Conductors $= \cdot42''$	Insulation :— Conductor to Sheath $= \cdot72''$ Between Conductors $= \cdot72''$
(1)	Carrying power with 5 to 7 % C^2R loss, ·8 power-factor..........................	2,500 K.W. 8 miles	2,000 K.W. 15 miles
(2)	Voltage between conductors $=$ V	10,000	20,000
(3)	Section of each conductor...	·21 □″	·076 □″
(4)	Capacity in M.F. per mile, one conductor to two others and sheath $= k'$ ...	·265	·152
(5)	Ditto, three conductors to sheath $= k''$	·5	·326
(6)	Resultant M.F. per mile for each conductor $=$ $K = \frac{9k' - k''}{6}$	·314	·173
(7)	Charging current per conductor-mile $=$ $C = \frac{2\pi n K V'}{1{,}000{,}000}$ $n = 40$ $V' = \frac{V}{\sqrt{3}}$	A ·45	A ·5
(8)	Apparent loss per mile for three conductors V′ $= 3\left(\frac{C \times V}{1{,}000 \times \sqrt{3}}\right)$	8 K.W.	17 K.W.
(9)	Total apparent loss for entire cable and % to full load	64 K.W. 2·6 %	255 K.W. 12·7 %
(10)	True loss if capacity power-factor $=$ ·124	0·32 %	1·6 %

The important dimensions in the cables for Cases I. and II. are given. The cables are of the form :—



Taking the details given in order—

(1) I am not sure of the exact distances over which the cables operate, but the carrying power and distances are given as understood by the size of conductors, voltage and power transmitted on the three-phase system, star-connected generators. These

sizes and distances are of special application to the Electric Power Acts, where no distance is likely to be greater than, or even to attain, fifteen miles. Mr. Cruise.

(2) 10,000 and 20,000 volts between conductors in the above system of transmission involves each conductor in a pressure of $\frac{10,000}{\sqrt{3}}$ and $\frac{20,000}{\sqrt{3}}$ volts respectively, above earth. It is this latter voltage that influences the charging currents.

(4) One might easily expect that the practical measurement k' was the capacity to be considered per conductor. On the other hand, it is obvious that the capacity phenomena in such a cable and system are complex, and here again it is that M. Guye has made such valuable researches, both practical and mathematical, and arrived at definite and simple results.

(5) M. Guye finds that k'' must be taken into account, namely, the capacity of the three conductors in parallel to the sheath. In the measurement under (4) the two conductors and the sheath were taken in parallel and measured to the remaining conductor. k' and k'' are measurements readily taken, and M. Guye has proved that

(6) The resultant capacity $K = \frac{9k' - k''}{6}$. K, it will be noticed, is less than k'' and greater than k'; it approaches most to k'.

(7) Here I have worked out the charging current per mile of each conductor. The periodicity has been taken at 40, as the likely figure for power transmission in this country.

(8) Thence is calculated the apparent k.w. loss per mile for the three conductors.

(9) This gives the total apparent k.w. loss over the respective lengths of cable under consideration.

It is clear that the real watts cannot be more than the apparent watts, but even if they were equal they would prove tolerably negligible in the 10,000 volt cable, which voltage, I think, is likely to be far more employed in this country and in the Power Acts than 20,000 volts.

(10) But if I take Mr. Mordey's factor ·124, although I do not know how far it is right to apply it to these cables, the real watts would be very small in both cases compared to the power transmitted.

Of course it must be remembered that the capacity loss is continuous, while the kilowatts transmitted have to be considered on a 25 or 30 per cent. load factor for a large power scheme.

Taking Case II. of the cables, *i.e.*, that for 20,000 volts, which is altogether the least favourable, there is

17 k.w. apparent loss per mile,
Say real loss $= 17 \times \cdot 124 = 2{\cdot}1$ k.w.

Take forty miles of transmission divided up into different circuits, and used in connection with, say, 7,000 k.w. of plant, and allowing 5 per cent. transmission losses other than capacity losses. Allow 30 per cent. load factor. We get

Mr. Cruise.

B. of T. units per annum	=	18,270,000
B. of T. units lost in capacity per annum	=	730,000
Total		19,000,000

Thus the ratio

$$\frac{\text{Units generated per annum}}{\text{Units lost in capacity effects}} = \frac{190}{7{\cdot}3} = 26$$

From this results that the units lost in capacity taken over a year would be some 3·8 per cent. of the total units generated for supply.

The worst possible conditions have been taken, and yet the loss comes out far less than with single-phase concentric cables, and is, generally speaking, well within economic bounds. It is equally certain that it would be far better to have this loss still less. That might easily be attained by keeping to the figure 10,000 volts as the working pressure, when the capacity loss would at once cease to reach a dangerous figure, in fact, it would be comparatively negligible.

I might add that the dielectric in the cables in Cases I. and II. was paper and fibre. But taking, as has been done, the capacity power-factor at Mr. Mordey's figure for rubber cables, it is all in favour of an excess estimate for losses in these three-phase cables. I would like to have Mr. Mordey's opinion on these figures and results, as, if correct, I think they have an important bearing on the question under discussion. There is, however, one important point to note. If the Board of Trade will only allow small loads, say 1,000 k.w. per cable, then the per cent. capacity losses will go up at an alarming rate, and they would eventually become very formidable.

For low power cables, say 500 k.w. and under, or somewhat higher powers for very long distances, the losses would become disastrous. In one case I have, calculated, where 10,000 volts was used to take 800 k.w. thirty miles on the three-phase system, if this transmission had been effected with underground three-core cables the capacity loss (·124 power-factor) taken over a year might easily attain 20 per cent. of the units generated. To this would also have to be added the other transmission losses (5 to 7 per cent.), making the whole transmission loss a hopelessly impracticable one.

Finally, I might remark that the continental engineers I have met with seem to fear resonance effects due to capacity and attendant insulation breakdowns, unless certain precautions are taken, far more than the capacity losses themselves. It is true that on the Continent experience has largely been with overhead transmissions; but it is nevertheless a fact that the first extensive high-tension transmissions through cables are also being done on the Continent, and therefore their experience is valuable. Apparently the figures shown here would tend to confirm one in the belief that capacity losses in well-designed power schemes will not be dangerous after all.

Mr. Baillie.

Mr. G. H. Baillie (*communicated*): It is a curious fact that among all the power-factors given by different speakers, the professorial are uniformly lower than the engineering ones. It may, perhaps, help forward the end preached by Mr. Swinburne, when the Professor and the Engineer will lie down together, if I mention some experimental

results obtained by a professor who in this respect is among the engineers. The experiments were carried out by Professor Lombardi, and I myself took some part in them at their commencement; they are published partly in *L'Elettricista*, 1896, and later in pamphlet form, but have not yet, as far as I know, been translated into English. The power-factor found for waxed paper was 0·011, confirming other results already mentioned; for two Swinburne condensers, 0·014 and 0·023; for two samples of small cables insulated with guttapercha, 0·037 and 0·068; for two samples of glass, 0·070 and 0·132, confirming Mr. Swinburne's previous results. The power-factor of 0·068 for guttapercha cable is certainly high enough to justify Mr. Mordey's contention that the loss of energy in cable dielectrics is worthy of attention, whether the cause of the loss be called hysteresis, leakage, or imperfect polarisation. Mr Baillie.

That some part of the energy loss is due to hysteresis, that is, to a time-lag between the stress and its strain, is conclusively shown by Arnò's results; but it is probable that some is due to C^2R losses, where R includes, not merely the arbitrary resistance as measured by continuous current, but also, to use Maxwell's analogy, the resistances between the elementary condensers of which an imperfect dielectric is composed. This view is strengthened by the results of Lombardi's experiments, which show that a large energy loss is accompanied by a correspondingly slow polarisation, for in the imperfect dielectric as imagined by Maxwell the interposed resistances would have the effect of making the polarisation slow. The waxed paper condenser, which had the small power-factor of 0·011, took up 97 per cent. of its maximum charge in the shortest time given by a Helmholz pendulum—something less than 0·001 sec. The guttapercha cable, with power-factor of 0·037, took up 90 per cent. of its charge in the same time; while glass, with the high power-factor of 0·137, took up only 5 per cent. of its maximum charge. Lombardi's measurements were not made with a view of establishing any relation between energy loss and rapidity of polarisation, and are not sufficiently numerous to do so; but if the connection they indicate be confirmed, I would suggest that the curves obtained in making the insulation test of a cable might afford a good indication, if not a measure, of the energy loss to be expected. The cable-makers have every facility for taking the curves, and these may well give a better idea of the energy loss than a wattmeter, which, according to the discussion, has an average error of 1,000 per cent.

In criticising Mr. Mordey's measurement of the power taken by the cable, it was generally assumed that his readings were too high because he had used a wattmeter; as a matter of fact, the difficulty is to make a wattmeter read high enough under the conditions of the test. Self-induction in the pressure coil of the wattmeter would tend to make its readings too low, unless, as Dr. Sumpner suggested, there was sufficient to make the wattmeter read more negative watts than there were positive. This, though theoretically possible, is out of the question in Mr. Mordey's case; supposing the true power-factor to be 0·025, in order to measure a negative power-factor of 0·124, the wattmeter must have the resistance in the shunt circuit only 3,500 times its self-induction

Mr. Baillie. coefficient; as this is of the order 0·001–0·01 henry, the resistance must be only 3–35 ohms, and I do not think such a wattmeter is on the market. As an alternative way of measuring negative watts in mistake for positive, Dr. Sumpner suggests transforming down the volts. This is more feasible, if the secondary volts lag more than 180° behind the primary. Mr. Duddell, however, in the *Electrician* of February 8th, says he has measured a lag of less than 180°. It is possible that the short-circuited secondary coil may prove a source of error, due to the series coil inducing currents in it, if the two are not exactly perpendicular.

The wattless power received but little attention in the discussion, probably because it is unimportant at the pressures common in England; at extra high pressures, however, it can assume startling proportions, even in aerial lines, where Mr. Mordey treated it as negligible. This may be seen in the case of the Standard Electric Company's 3-phase line, transmitting 150 miles to San Francisco at 60,000 volts, where the engineers expect a charging current of 32 amperes, representing some 3,300 wattless kilowatts. If cables were substituted for the overhead line, the charging current would be about 20 times the full-load current.

Mr. Whalley. Mr. A. WHALLEY: Mr. Mordey's paper has a special interest to me personally, as I believe his attention was first drawn to the importance of capacity-current in connection with a large network of 2,000 volts concentric rubber cables installed at St. Petersburg by the Helsby Company, for the design and laying of which I was responsible. There are now about 250 kilometres of cable laid, having a total capacity of about 88 M.F., the capacity current being theoretically 47 amperes. It has, however, been recognised since the question was raised that instead of this current being a disadvantage, it is of service in partly neutralising the wattless current of the transformers. So large is the latter, however, that the capacity-current would have to be five times larger than at present to neutralise it. I am of opinion that in another direction the so-called high capacity is of value, as there has been an entire absence of breakdown of the cables on the present network, while preceding cables of much lower capacity gave considerable trouble; the breakdowns—as is generally the case—occurring at times of light load.

From tests made on Helsby Rubber Cables—using, through the kindness of Professor Ayrton, the same instruments which were recently used elsewhere—it is found that the losses in the dielectric indicate a power-factor well under 3 per cent. With the same cable the results vary with the alternator used, and may be reduced by using transformers. A power-factor of ·0255 was obtained off an alternator at 103 $\sim$, with choking coil and cable in parallel; while using another alternator at 50 $\sim$, without a choking coil, a power-factor of ·0238 was obtained, and of ·0228 when transformers were used between the alternator and cable.

It is of interest to note that the condition for a choking coil to balance a given capacity is that of resonance. Fig. M (K.032) shows capacity current and P.D., for a cable of ·388 M.F., fed by a particular alternator direct, when the condition of resonance is being

Mr. Whalley.

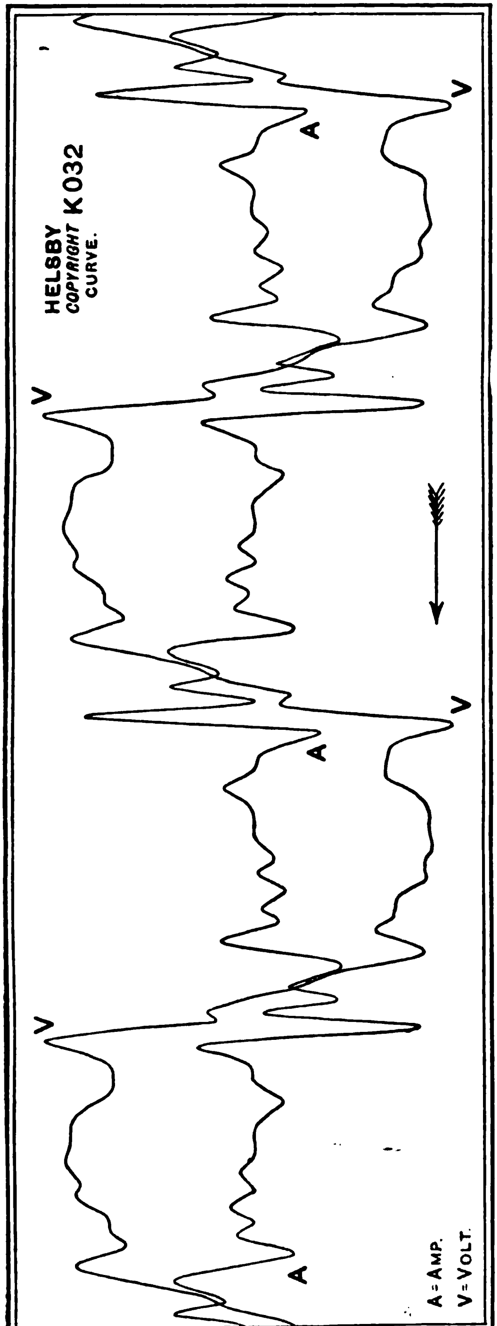


FIG. M.

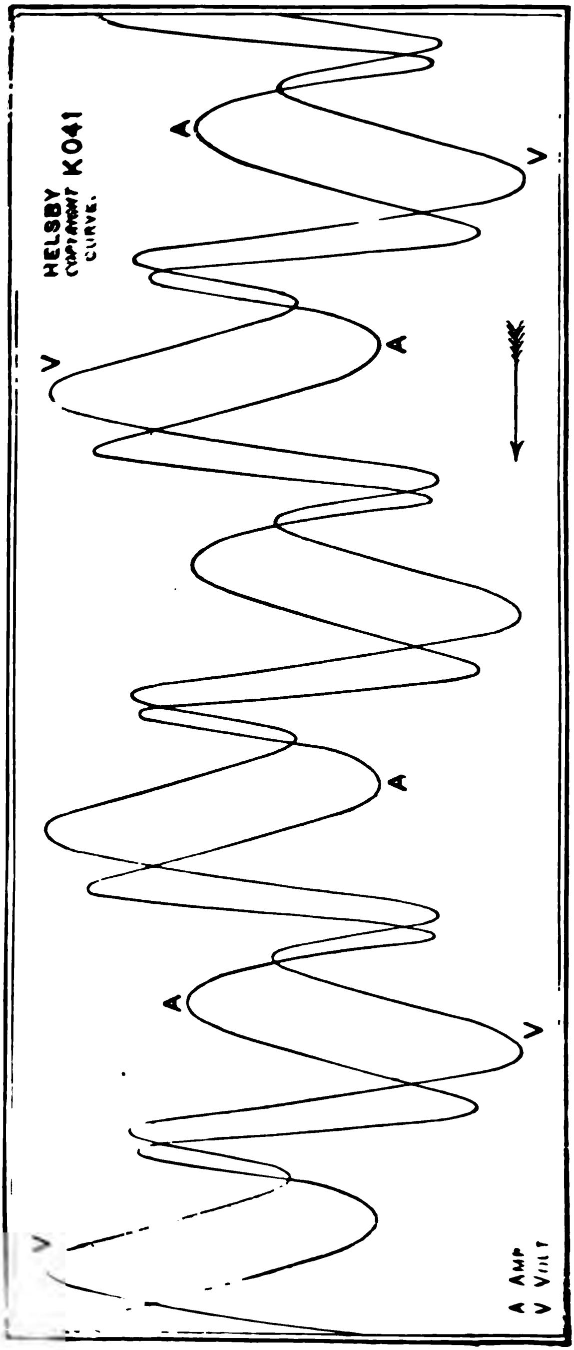


FIG. N.

Mr. Whalley.

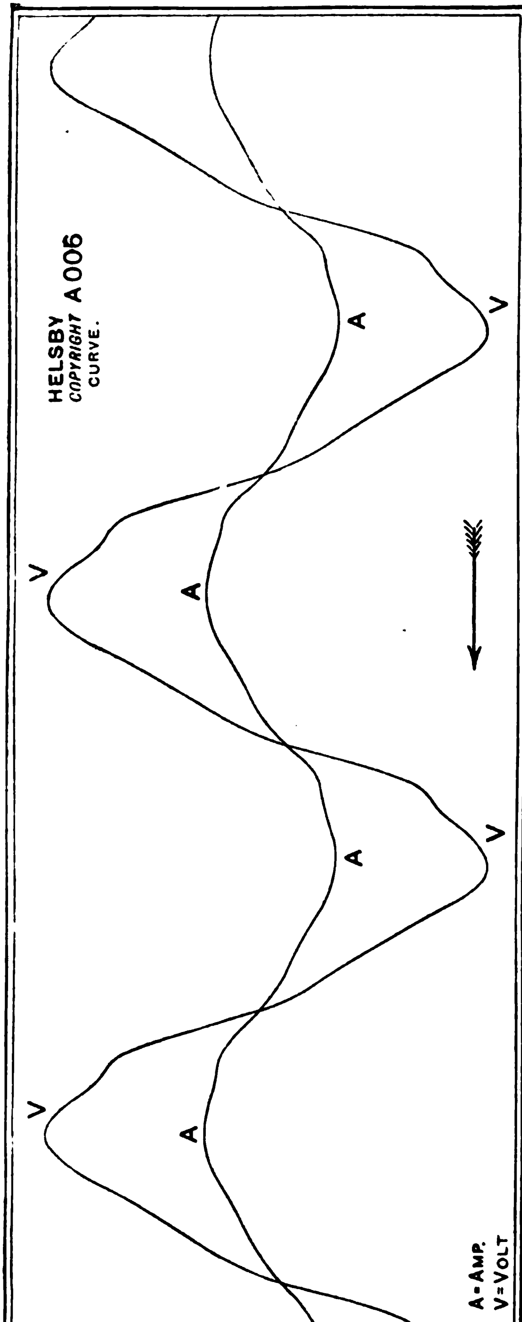


FIG. P.

Mr. Whalley.

approached. The P.D. is 2,000 R.M.S. volts, and the current 0·2 R.M.S. amperes. Some caution may be needed before using a network in a condition of resonance until it is known if there is then any special risk of the breakdown of insulation.

Fig. N (K.041) shows the capacity current and P.D., with the same alternator working on a cable of 4·90 M.F. at 1,980 R.M.S. volts, the capacity current being 6·04 R.M.S. amperes. These photographic records, and the power-factors quoted, show that the peculiarities of the alternator to be used under a capacity-load ought to be known when considering the adoption of choking coils or the value of the power-factor of a dielectric.

The same machine under a transformer load gives the curves of P.D., and current shown in Fig. P (A.006), at 2,040 R.M.S. volts, and 29·5 R.M.S. amperes, when the machine is connected direct to the transformers to avoid capacity in the primary circuit, and the secondary load of the transformers is practically non-inductive.

Mr. Spark's comparison of the capacity of paper *versus* rubber cables has apparently been made on the assumption that the paper cable would have about twice the thickness of insulation of the rubber cable tested by Mr. Mordey. For the same thickness of insulation the ratio of the two capacities is generally taken as being 2 to 3, but it would be wrong to assume from this that every paper cable has a less capacity than rubber, as a very small percentage of moisture left in the oil or paper, or absorbed after manufacture, considerably increases the capacity.

The heating of the German three-phase cables on light loads, mentioned by Mr. Mordey on the authority of Mr. Kapp, requires further elucidation from a cable-maker's point of view. Such heating, when obtained in factory tests, is generally explained by a local fault or extremely low insulation throughout.

Mr. O'Gorman raises some very interesting points, and his curves would be of great value if they were the result of actual tests; in fact, they are described as though this were the case. I believe that they are simply calculations, however, and that at the present moment the deductions drawn from them are theoretical and have still to be proved in practice.

Mr. Andrews.

Mr. L. ANDREWS: In spite of the evidence given by the majority of those who took part in the discussion upon Mr. Mordey's paper, which appeared to show that capacity-current losses in high tension are practically *nil*, I still think that the cost of generating current for charging cables is a very serious item of expenditure in many alternate current stations.

At Hastings, for instance, between the hours of midnight and 4 p.m. during the past year we have generated 188,000 volt-ampere units, and for this purpose have used about 840 tons of coal. The above output may be analysed as follows:—

Useful work	94,600 units.
Exciting transformers	35,000 "
Capacity current	58,400 "
Total	188,000 units.

Now, I am convinced that if we had no high tension cables to charge, but merely had to generate the 129,600 actual units per annum, we should not use more than 10 lbs. of coal per unit generated. On this basis the actual units generated would account for a coal consumption of 579 tons per annum, leaving 214 tons, costing £359, to be accounted for. This, we believe, represents the annual cost of charging the high tension cables during the hours of light load. Whether it is wasted in dielectric hysteresis, or whether it is entirely due to the fact that for several hours per day we have had to run larger, and, as it so happens, less economical plant than would have been needed for generating the actual units, is not known. I merely wish to show that ours is a case in point where the generation of capacity currents does apparently cost between £300 and £400 per annum. Mr. Andrews.

We have for some time been taking steps to prevent the above loss by arranging our system of distribution in such a manner that it will be possible to disconnect the whole of the high tension feeders and transformers during the hours of light load, and feed the few consumers we have on at that period through the low tension distributing mains. When we have this completed, we shall be able to run for a week with our high tension feeders connected, and for a week without, and so be able to make careful comparisons which will show us the actual cost, under working conditions, of generating capacity currents.

Mr. W. DUDDELL (*communicated*): As I am responsible for the statement in Mr. Minshall's remarks that the Swinburne wattmeter, used in the tests marked thus †, was specially calibrated on a power-factor of 0·1, I should like to make a correction. The wattmeter itself was tested on a low power-factor and found satisfactory; but *the wattmeter in conjunction with the* 20,000 *ohms* series resistance, as used by me at Croydon, *was not so tested*. The 20,000 ohms resistance consisted of two 10,000 ohm coils made by Mr. Swinburne and wound in the manner described by him at the meeting. Mr. Duddell

In trying to find out whether the high power-factor of the Croydon cable was due in any way to its apparently low insulation resistance, I tried the effect of varying the frequency, when I noticed that at very low frequencies there was a distinct clicking sound in the 20,000 ohm resistance, which I traced to a brush discharge occurring between the layers of wire on the coils at each peak of the wave.

This brushing, which I noticed at the very low frequencies, may also be taking place at the ordinary frequencies, and leads me to doubt the accuracy of the tests taken with this Swinburne wattmeter when using the 20,000 ohm resistance in series with the P.D. coil. Further experiments have shown that the wattmeter always reads too high when used, with these particular coils, to measure the power supplied to a circuit in which the current leads, even when no brushing occurs, owing to the capacity effect between the different layers of the coils.

Mr. Mordey and Mr. Mather have both drawn attention to the effect of wave form on the capacity current of the cable, and it is interesting in this connection to note that when using alternators having wave forms very different from a sine curve, such as those at Croydon, the frequency of the cable current is several times the frequency of the

[illegible]

[illegible] ... the measurement of the [illegible] ... dielectric hysteresis, the measurement of [illegible] ... unnecessary in the case of the [illegible] ... in consequence of the methods [illegible] ... the current which he supplied [illegible] ... arrangement [illegible] ... with the P.D. [illegible] ... power [illegible] ... from the readings of the [illegible]

[illegible] ... represents one of the many tests carried out [illegible] ... of the British Insulated Wire Company, [illegible]

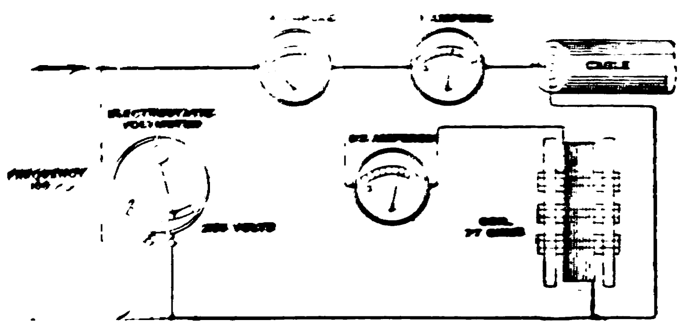


FIG. 9.—Current Supplied less than One-Fortieth of the Sum of Currents in Branches.

Now the total power given to the cable and to Mr. Mather's choking-coil *could not have exceeded* 0·3 × 2,150 or 645 watts. The power given to the coil alone *could not have been less* than $\overline{6{\cdot}2}^2 \times 7{\cdot}7$,* or 296 watts. Hence the power given to the cable *could not possibly have exceeded* 645 − 296, or 349 watts.

The volt amperes for the cable were obviously 2,150 × 5·9, or 12,685; hence the power-factor of the cable *could not possibly have been lower* than $\frac{349}{12{,}685}$, or 0·028.

This value, 0·028, is, of course, an outside limit, and therefore is higher than the actual power-factor 0·024 published by Mr. Mather for

* The [illegible] was measured with a Wheatstone bridge, just be[illegible] [illegible]

this paper cable, but that the two values differ so little is a striking proof of the accuracy of Mr. Mather's figures. Professor Ayrton.

In my remarks made on the 10th of January I promised that I would construct a coil *without iron* which, under Mr. Mordey's conditions as regards capacity, frequency, and P.D. should waste far less energy than did his choker, and should be able to reduce the current in the supply leads to a fraction of the least value which he was able to arrive at. Comparing the readings of the instruments in the preceding figure with those in Fig. 4, page 378, of his paper, and referring to the particulars of the weights of their coils given respectively by Mr. Mordey and Mr. Mather, we see that the comparison is as follows :—

	Mordey.	Mather.
Least current in supply leads, amperes...	1·6	0·3
Power in watts wasted in choking-coil	500	296
Weight in pounds of complete choking-coil	260	92·5

Mr. Sparks has told us that in carrying out the test described in Mr. Mordey's paper, the energy-meter rotated *backwards* when applied to the cable. To be logical, therefore, Mr. Mordey ought to have concluded that a cable was a *source of energy*. But I presume that whether the meter ran forwards when applied to the choking-coil alone, or backwards when joined with the cable alone, the reading was in each case assumed to measure energy *given to* the coil or cable respectively.

Directly, however, that the proof of Mr. Mordey's paper was sent out, and before the paper was read, some of us suspected that the energy-meter in his experiments did run *backwards*. And so Mr. Evershed, Mr. Mather, Mr. Duddell, and myself considered independently what an energy-meter attached to a cable really indicated when it ran backwards; and the result, which has been tested experimentally in my laboratory, is as follows :—

Let h and o be the self-induction, in henries, and the resistance, in ohms, of the pressure coil of an Elihu Thomson energy-meter, that is of the armature, starting coil, and so-called non-inductive resistance; then such a meter attached to a cable, as in the accompanying figure

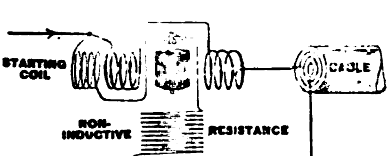


FIG. R.—Thomson Energy-Meter.

Professor Ayrton.

and supplied with current following the sine law, *will rotate forwards, or will not budge, or will rotate backwards, according as the true power-factor of the cable exceeds, or is equal to, or is less than,* $\frac{hp}{o}$.

Hence, apart from the rate at which the energy-meter runs backwards, or the voltage applied to the cable, or what current may flow into it, the mere fact that the meter *does* run backwards proves, with sine waves, that the ratio of the *true* power to the volt-amperes, that is the *true*

power-factor of the cable, is *less* than $\frac{hp}{o}$.

Next, measurements were made on four modern specimens of Thomson energy-meters, for the loan of three of which I am indebted to the British Thomson-Houston Company. And the following were the results obtained:—

No. 62,695	$h = 0{\cdot}25$ henry	$o = 3{,}785$ ohms.
,, 230,355	$h = 0{\cdot}24$,,	$o = 2{,}100$,,
,, 230,375	$h = 0{\cdot}26$,,	$o = 3{,}060$,,
,, 230,380	$h = 0{\cdot}22$,,	$o = 3{,}095$,,

Hence, if the meter illustrated in Fig. R be the first one in this list, for example, and if, when a sine wave current of 100 $\sim$ be applied, the meter be found to run *backwards*, then, no matter at what speed it may run backwards, or what may be the value of the voltage applied to the cable, or the current flowing into it, the *true*

power-factor of the cable is *less* than $\frac{0{\cdot}25 \times 628}{3{,}785}$,

i.e., is *less* than 0·042.

And this limiting maximum value, curiously enough, is not so very much greater than the actual value found by Mr. Mather for this County of London cable, viz., 0·029.

Further, it can be proved that the *faster* the energy-meter is found to run *backwards* the *smaller* is the *true* power-factor of the cable, that is, the less is it compared with this limiting value 0·042.

If instead of an energy-meter, a simple wattmeter be employed, then for running forwards, staying at rest and running backwards, we have merely to substitute a positive deflection, deflection nought, and a negative deflection. So that with a simple wattmeter applied to a cable which is supplied with a sine wave current, the wattmeter will give a *negative* deflection (as obtained, for example, by Dr. Sumpner), when the *true*

power-factor of the cable is *less* than $\frac{hp}{o}$,

h and o now referring to the fine wire circuit of the wattmeter.

As the preceding energy meters can only be employed directly on circuits with a pressure of 200 volts or less, it is most probable that Mr. Mordey, in his tests, used some form of step-down transformer, the effective inductance and resistance of which would have to be included the preceding formulæ. But no information has been given, either the paper or in the discussion, regarding the size, shape, resistance,

or inductance of the transformer, nor are we even told whether such a step-down transformer was actually employed. Professor Ayrton.

Mr. Sparks was not present at the meeting on the 10th of January, and so has been misled into thinking that on that occasion I said that if measurements were accurately made with the County of London Cable, the value found for the "loss of energy would be one-tenth of the figure given." No such statement is contained in the shorthand writer's official transcript of the verbatim report of my remarks, nor is there any such statement in the account of the discussion as given in the technical press at the time.

Perhaps Mr. Sparks had in his mind what I said about the value of the power-factor, 0·01, found some years ago in my laboratory for the Swinburne butter-skin condensers. If so, it is interesting to note that Mr. Swinburne now remarks that this value, 0·01, is not lower, but considerably *higher* than he himself found for the same type of condenser.

Mr. W. M. MORDEY, in reply: I will not detain you whilst I attempt to reply to all the speakers: I will make one or two observations only. Mr. Mordey. I am very glad the last speaker, Mr. Minshall, has given his experience, especially as in his tests of the 5,000-volt Croydon cable he has been assisted by Mr. Duddell. His power-factor of 0·1 is very near my own, and supports the belief that there are appreciable losses in cable dielectrics. A further practical confirmation has reached me from Mr. Kapp, our old member, who has written me from Germany, giving a singular confirmation. They find high-tension three-phase cables in Germany get appreciably warm on light load. An appreciable warmth, of course, means a good deal of power. Such an observation is very important. We have been told by Professor Fleming that we must use a motor generator test. We have done so. Mr. Sparks has used a motor generator test, and Mr. Minshall has, and they have found a large addition of power required to drive the motor when the cable was switched on. This method Mr. Sparks shows, also gives a power-factor of more than 0·1.

I should like to recall a statement made by Mr. Swinburne about ten or eleven years ago at the Physical Society (see *Electrician*, Dec. 19, 1900), when he said that from his calculation of the losses in paper condensers, the loss in Deptford paper mains from dielectric hysteresis would be about 7,000 watts. Curiously enough, Mr. Partridge, who did not know of this, and who is the engineer of the Company, assures me that the loss in these cables is quite 1,000 watts per mile, or exactly the figure that Mr. Swinburne gave ten years ago. Some of the figures given, even the figures that have been quoted by Professor Ayrton from Lombardi and Cardew, confirm me to a certain extent; they are in the same order. Lombardi's results come out at 0·068. That is in the same order as the losses that I am considering. It is not, as Professor Ayrton and others have tried to show, a mere fraction of that amount—a mere negligible amount undeserving of serious consideration. With some kinds of cable the power-factor may be lower, and with other kinds of cable it may be much more. Another point: cable makers have told me that they have often had

Mr. Mordey cables which on light load got quite warm to the touch, simply from dielectric hysteresis—cables that have a good insulation resistance.

We have had an exceedingly useful discussion, I consider, to-night. Quite possibly I may turn out to be wrong to some extent. For the sake of the industry I shall rejoice to find that I am very much wrong. Of course I need not say that I shall be very sorry for personal reasons if I have made a big mistake. I do not think I have. There are many practical indications that I have not—such, for example, as the known and considerable vibration of alternate-current cables. Such a vibration can hardly occur in a heavy cable without considerable loss of power.

May I say that I never claimed for one moment that the power-factor found in that cable, and now apparently confirmed by the motor generator method, was of a universal application. I repeatedly took pains to guard against such an impression. I knew, of course, that I was building general conclusions on the results of one test, and I repeatedly stated in my paper that *if* the results I got were correct, and *if* they applied to other materials and other kinds of cables, the loss of energy would be as I made it out to be.

I want to repeat my thanks to many speakers, especially to Mr. Sparks, who took up the subject as a serious engineering matter, and in his Company's interest made as full an examination as he could with the unusually complete means at his disposal. Personally I am very much obliged to him, and I think the Institution ought to be very much obliged to him for the trouble he has taken in this matter. I beg to thank the gentlemen who have done me the honour to speak on the paper, and I hope to deal with their remarks later on.

[*Added by Author.*]—I am very much gratified to find that my paper has brought out such a mass of useful information. The contributions to the discussion seem to me to be very valuable. It would not be possible for me to deal with them in any detail, even if I were able to do so. Fortunately most of them can stand by themselves as useful additions to the knowledge of the subject apart from their relation to my paper. I have already alluded briefly to Professor Ayrton's contribution. He misquoted Cardew and Lombardi in his effort to prove that I did not agree with him. I have pointed out that his claims to early knowledge must not be taken very seriously, seeing that up to the reading of my paper he never realised the very great importance of wave form in questions of capacity current, but boasted of having for ten years used (presumably for teaching purposes) methods which are now known to be quite unreliable in practice. Although right enough on the blackboard, they were wrong by two or three times (or by two hundred or three hundred per cent. as he would picturesquely describe it) in practical applications. From Mr. Sparks' remarks I learn that in another test case Professor Ayrton was hopelessly wrong. He pointed out with great emphasis that my choking-coil arrangement for reducing capacity current generated by the alternator was very imperfect inasmuch as it only reduced the current from 6 amperes to 1·6 amperes, and he offered to supply a choking coil which would reduce it to 0·07 amperes. Professor Ayrton points out that the proper method was all clearly given in a paper by him

before the Physical Society in 1891, and he proceeds to criticise my result thus (quoting from the shorthand report): "But now if we take Mr. Mordey's own experiment, I say that by dealing with the actual values given in that paper without going a step further—and that is the best proof of what was in that paper—I can enormously improve his result." He then shows that the current will be reduced to 0·07 by his coil instead of 1·6 by mine. Well, Mr. Sparks has told us that Professor Ayrton made this test with his own coil and brought the current down not to 0·07, but to 2·7 amperes. **Mr. Mordey.**

Thus, instead of reducing the current to about 4 per cent. of mine, by his coil he made it about 70 per cent. greater than mine—a forty-fold error—or as Professor Ayrton would express it, an error of four thousand per cent. Truly as he says, "Without going a step further, that is the best proof of what was in that paper."

It is, perhaps, unfortunate that on these two simple matters the tests which have been made have been so entirely against Professor Ayrton's conclusions. It has rather discouraged me from considering the rest of his communication as carefully as it no doubt deserves; but at least it stands in the Journal for reference.

Professor Ayrton's further communication (p. 460) is a filling out in great detail of what I pointed out briefly at p. 378—viz., that if a choking-coil be used to balance the capacity, and if the balance is perfect, then the alternator will be working with a power-factor of unity. Professor Ayrton shows that the watts cannot be greater than the volt-amperes.

Mr. Sparks' account of the tests that have been made on his cable will be read with interest by all who may wish to study this matter seriously. The discrepancies obtained in the dielectric losses show the difficulty of the subject. The motor-generator tests seem to fully confirm the original wattmeter tests. If they are too high, it can only be because of eddy current losses in the generator due to the charging current. This source of error would be eliminated or reduced by using a choking coil to balance the capacity so as to reduce the charging current required to be produced by the generator.

Mr. Sparks' observations on the discrepancy between the calculated and the observed capacity current confirms what I have already said, and further emphasises the need for stating definitely what is meant by the term "power-factor." As the "apparent watts" may vary enormously while the true watts remain fairly constant, it is evident that the term power-factor must be used with caution, otherwise misleading comparisons may be made.

I am glad Mr. Sparks has brought out so clearly the practical importance of dielectric hysteresis. He shows that even if the lowest power-factor found in the tests—the lowest of Professor Ayrton's or Mr. Mather's results—is the correct one, the annual loss on his cable will be 155,000 units, or 8·3 per cent. of the total units delivered. At a penny a unit this amounts to £645 a year, or a capitalised value, at 5 per cent., of £12,900. Mr. Sparks very properly points out that with another kind of dielectric the loss might be only one-third of this. True, but do not let us forget that with a two-phase cable the loss

Mr. Mordey. would be doubled, and with a three-phase cable it would be increased to three times that of a single-phase. This difference between single and multiphase working seems to merit some attention.

It is to be feared that Mr. Sparks' remarks support the view that dielectric hysteresis, considered practically, is a cause of serious loss and is deserving of careful attention.

Dr. Fleming's former connection with the Deptford system makes his remarks especially interesting to me. Knowing the great difficulties of wattmeter measurements under the conditions dealt with in my paper, he prefers a simple motor-generator test to any purely instrumental methods. He thinks such a test must give accurate results. It will interest Dr. Fleming to see that Mr. Sparks' motor-generator test, as well as that of Mr. Minshall, fully confirms the large power-factor. If the motor-generator result is not entirely accurate, it must be, as I have pointed out above, that the capacity current causes reactions in the generator which lead to losses in eddies and magnetic hysteresis.

I am interested in Dr. Fleming's account of his own efforts to measure the losses in the Deptford cables by taking indicator diagrams of the engine with various lengths of cable in circuit, but I am afraid I cannot agree that the method is likely to give accurate results. The engines available were of large size, and their indicator cards under very light-load conditions would be of such a character that it would be practically impossible to detect the small power mentioned by Dr. Fleming.

In connection with the Deptford mains, I have the permission of Mr. G. W. Partridge, the Company's engineer, to state that the actual loss of power in their mains is about 1 kilowatt per mile. As there are 28 miles of mains, having a capacity of about $\frac{1}{3}$ mfd. per mile, this works out for the 28 miles at 10,000 volts 87 $\sim$ to 18,215 apparent watts per mile (assuming sine-curve E.M.F.), and on Mr. Partridge's figure of the true loss to a power-factor of 0·055. If the whole 28 miles were always in use the annual loss would be 245,280 units. Mr. Partridge, however, reduces the loss to much less than this by reducing the number of mains in use during hours of light-load, thus saving both the true loss and the indirect losses due to the production of "wattless" current. This, it will be seen, would require fully 500 k.w. of plant unless compensated in some way.

Dr. Sumpner says I should state the dielectric loss as a percentage of the load the cable is to transmit. To do that one must know the load. Dr. Sumpner works out the County Company case and makes the loss something between $\frac{1}{4}$ per cent. and $1\frac{1}{4}$ per cent. Mr. Sparks, the engineer to the Company, works it out from a practical knowledge of the load and assuming the lowest power-factor obtained in any test referred to in this discussion, and finds the loss will be 8·3 per cent.

Let me give a practical case that has just come under my notice. A 50 $\sim$ 10,000-volt three-phase cable, 10 miles long, is proposed as a means for supplying a lighting load having a maximum of 200 k.w. and a load-factor of 12 per cent. The cable is expected to have a capacity

of 0·3 mfd. per mile. Assuming a power-factor of only 0·025 (or lower than any one has yet found in a cable), the dielectric loss will be 24 per cent. of the units transmitted. This, of course, shows how unwise it would be to use such a cable for such a load. Mr. Mordey.

I am much interested in Dr. Sumpner's remarks on transformers in connection with wattmeters, although I am afraid I do not altogether follow his argument. He refers to his use of a large transformer "very under-loaded," but surely he knows—what Dr. Fleming taught ten years ago in his well-known paper on transformers—that a transformer fairly well loaded had a power-factor of 1, and kept it up to full load. It was for that reason that in arranging my wattmeter I purposely

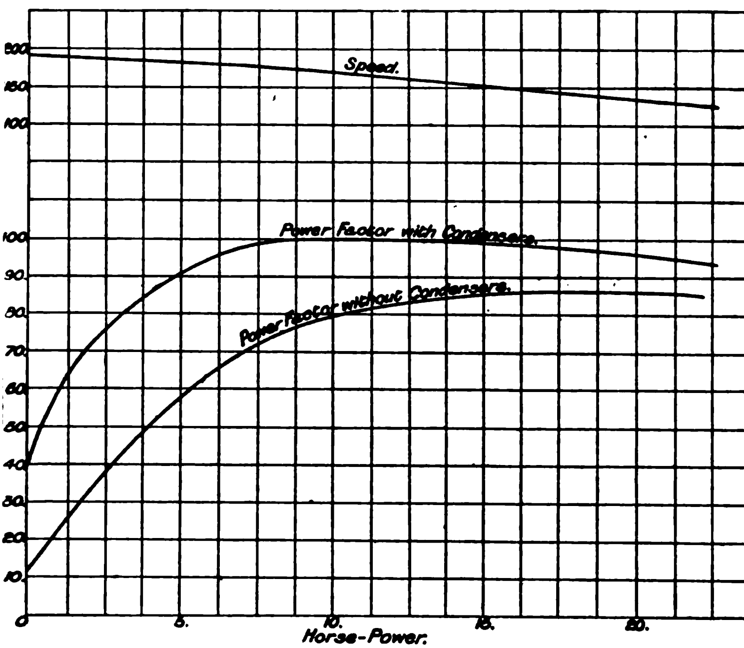


FIG. S.

made the short-circuited suspended secondary of such a character that the transformer was fully loaded.

Mr. Swinburne, in his remarks, is interesting and suggestive as usual, but I do not think there are any points that call for a reply from me. Mr. Swinburne made gallant efforts to introduce condensers into practical use a good many years ago; it will interest him and others to know that Mr. Wm. Stanley, the well-known American electrical engineer, is supplying condensers in considerable quantities for improving the power-factor of alternate-current motors. Mr. Stanley informs me that there must be over 100,000 H.P. of motors working in that way in the United States, and he sends me some interesting

Mr Mordey. curves showing results, one of which I give here (Fig. S). The importance of this result will be at once recognised.

I have discussed with Mr. Stanley the questions raised in my paper, and learn from him that there are very considerable dielectric losses in many cables, and that the reduction of those losses in the dielectric of his condensers occupied him for a very long time. He eventually succeeded in his efforts to make condensers on a commercial scale, in which the losses are very small. The very slight heating of the condensers is a proof of this.

From another eminent American electrician, Mr. Steinmetz, I hear indirectly that in at least one "extra high tension" transmission line in the United States where there is some underground cable it has been found that there is a serious dielectric loss.

Mr. Mather seems to have gone very fully into several of the points raised in my paper, and is apparently of Professor Ayrton's opinion that my paper is only useful as a text for a paper of his own. For my part I am satisfied even to be a humble text, if by that means I can bring out such useful sermons. But the tone of the sermon is not conducive to discussion, so I gladly let it stand on its merits, which are considerable.

Mr. O'Gorman suggests that the heating of the cable mentioned by Mr. Kapp may be due to local failure of insulation on the outer layers of the cable, and that the heat may only be on the outside, the cable not being heated throughout. This would not affect the result. All the heat, however generated, must get out by the surface, and under long continued working the surface temperature would be the same for the same loss of energy, whether the heat came from the centre or the outside of the cable.

Mr. W. E. Gray refers to several points of great interest on which at present there is very little available information, such as the variation of capacity, and the effect on capacity of treatment of the dielectric during manufacture, and the possibility of getting low capacity with rubber or high capacity with materials that are supposed to have a low specific capacity. I can confirm a part of this even from my own small experience, as I came across a large paper-insulated cable, quite recently supplied, which had quite as high a capacity as the rubber cable on which my tests were made. Apparently the kind of paper and the material used to permeate the paper affect the result very considerably. Now that this subject has come up in a practical way it is to be hoped that we shall gradually get more knowledge on the essential points, and gradually reduce any causes of loss that may be under control. Cable makers will gladly co-operate with engineers to this end. I can say this with confidence from the way Mr. Gray and other cable makers—Messrs. Glovers, Messrs. Siemens, and the British Insulated Wire Co.—endeavoured to help me in my efforts to collect information on the points I have been trying to elucidate.

Mr. Threlfall seems to be one of the very few men who have worked successfully at the construction of condensers capable of standing high pressures. His remarks will be read with interest. He objects to my ascribing the energy losses to dielectric hysteresis, but he does not

help me much, as one of his conclusions is that "the cause of the dissipation of energy is not known." I gather that, like Mr. Swinburne, Mr. Threlfall thinks part of the loss may be due to a kind of conduction. This is a question of pure physics, on which I fear I am not very competent to speak, but I would refer Mr. Threlfall to the passages near the end of my paper where I tried to express a similar view. I point out that the energy component of the capacity current "in all practical essentials is a leakage current: it goes right through the dielectric, and heats it in its passage exactly like a leakage current." (See p. 383.) Mr. Mordey.

I used the term "dielectric hysteresis" under compulsion, as the only current electrical term to cover broadly the dissipation of energy in insulating materials, but I do not like it, and much prefer to use a non-committal term, such as "loss in the insulation."

Mr. Minshall's tests at Croydon tend to confirm the serious character of the dielectric loss. I almost wish they did not, as it would then be easier for me to say that I think we have in Mr. Minshall a man of exceptional ability and knowledge. His experiments are very interesting and very important. The effect of capacity in increasing the current is new to me. On that and other points many of us would be glad to hear more. I hope this Institution will obtain from him a full account of the work of which in this discussion he has given only enough to excite our curiosity.

Mr. Fricker's suggestion that dielectric losses should be taken at different periodicities will, I hope, be acted on. The pendulum illustration seems an apt one.

Mr. H. M. Sayers' experience of breakdowns and their reduction by change of frequency is very interesting and important. As very few people have had a wider practical experience than Mr. Sayers, his remarks on possible causes of cable breakdowns and on other matters will be found useful and suggestive.

Mr. Stuart A. Russell's conclusion from an examination of Professor Ayrton's paper on "Interference with Alternating Currents," is the same as my own. It appeared to me to be a collection of undigested purposeless experiments that at the time taught nobody more than it taught its author. Mr. Russell is also correct in pointing out that in 1891 neither Professor Ayrton nor any one else in the discussion on Dr. Fleming's paper showed that it was not necessary to go on generating to 45 amperes capacity-current at 10,000 volts.

Mr. Esson is in reality in agreement with me about the "Ohm's law" of the relation between pressure and capacity. The formulæ quoted as showing pedagogic knowledge of principles are based on nothing more substantial than a supposed necessity for some basis for blackboard calculations. I quite agree with Mr. Esson—these relations are "very imperfectly understood." It is worse than mischievous to express them as laws.

In *Mr. Alexander Russell's* remarks on the different wave effect of condenser and choking coils, he illustrates once more the difficulties of getting a perfect balance. He suggests that condensers should be reduced in size by transforming up; this seems sound, but I cannot

Mr. Mordey. find that it is ever done. I wonder why. It ought to be useful with low-tension motors.

Mr. Addenbrooke thinks the Board of Trade should allow overhead wires. Probably they would be allowed for crossing quite open country, but we are not likely to see them again in towns. His tests on losses in condensers are interesting, but not necessarily any guide to losses in cables. There seems little doubt that condensers may be made with very small losses. But if the same care were applied to cable-making, the price of the cable would probably go up very much.

Mr. Addenbrooke's researches seem to have covered a very large field during the last few years, but I can only refer to a few of the points he raises. We must make shift with cables, as it is pretty certain the Board of Trade will not allow overhead wires, except, perhaps, in the open country. The losses in condensers are probably very little guide to what occurs in cables. Even if the materials are the same, the methods of manufacture are not.

The choking coil took a current of about 0·15 ampere. I did not measure it very carefully. The eddy current losses in chokers with open gaps seem to be considerable. They disappear when the gap is closed. I think I explained that I used an iron choker with a gap to be able to put it in a reasonable-sized iron case. For practical purposes the external field of an ironless choker is a difficulty—and it is not adjustable easily.

Mr. Boot confirms my observations as to the checking of an engine when an idle cable is switched on. I hope he will find an opportunity of making the steam tests he mentions. His estimate of the cost of wattless current as about one-fourth of wattful current is about the same as my own. He has no doubt given the right explanation of the change produced on his power-factor by increase of length of cable.

Mr. Nisbett speaks as a practical cable maker. I am very glad he is taking such a keen interest in this matter. No one has better opportunities for experiment and investigation. Whatever result my paper may have, it cannot be an ill one for paper cables. Mr. Nisbett says Deptford could not lose much energy in its mains without knowing it—therefore the power-factor must be low. But Mr. Partridge has told me that he has found considerable losses in his idle mains, and therefore only keeps enough of them in circuit for the load. Then it must not be forgotten that in an ordinary alternate-current station a difference of 30 per cent. often exists between the "units generated" and the "units sold." If this can occur at 2,000 volts, what may not happen at 10,000 or 15,000?

Mr. Nisbett inclines to the belief that the power-factor in his cables is about ·025. I hope he is right. Let us assume, for the sake of argument, that he is right; and let us further assume that he can make cables with the very low capacity of 0·25 mfd. per mile. What will this mean in a three-phase, 10,000-volt, 50 $\sim$ cable? It will mean a loss of 589 actual watts, or 5,150 B. of T. units per mile per year. At 1d. er unit this eats up £21 9s. a year. The loss would be the same as if .p. lamps were placed nine yards apart along the mains.

r. Nisbett works out the case of the Midland Power Company's

cables, and shows that assuming all the 5,000 k.w. of plant is taken up, Mr. Mordey.
the percentage loss will only be 1·2 per cent. He kindly sent me particulars of those same cables. I make the loss 1·5 per cent., but that is near enough. The basis of comparison seems rather a dangerous one from a business point of view. After all, it does not take many small percentage losses to absorb an ordinary dividend, and 1·5 per cent. is 30 per cent. of 5 per cent. Till the whole 5,000 k.w. is taken up it will be more than 30 per cent. of 5 per cent.! On the important question whether the power-factor diminishes or increases with increase of pressure, I note the difference of opinion between Mr. Nisbett and Dr. Hoor; but he does not give any reference to Dr. Hoor's results, which were not contributed to this discussion. The only test I have been able to make is that given in Fig. 1. The straightness of the "curve" suggests that the power-factor is constant at least for pressures up to 4,500 volts.

I quite agree with Mr. Nisbett that chokers to reduce capacity-current generated are only useful at times of light load. Unfortunately light load often occupies a large part of the day.

Mr. Nisbett concludes by asking where the demand for condensers is to be found. The curves I have given of their effect on motor power-factors (p. 467) will answer this question.

Mr. Cruise's contribution to the discussion deserves careful study. As he is engaged practically in connection with extra-high-tension work his opportunities for obtaining information are probably considerable. As to the amount of the losses, Mr. Cruise is now in possession of all the information I can give him, and can very well form his own conclusions. He regards a loss of 9 or 10 per cent. as only "rather formidable," but thinks it would not quite spoil a dividend: I should have thought it would have wiped it out altogether. He assumes a large load and a high load-factor, and thinks it has been satisfactorily demonstrated that these things may reasonably be expected. He assumes that in power schemes 2,500 k.w. per cable will be common. I quite agree with him that if all these desirable conditions are obtained all will be well. He builds his hopes on it, but admits that if it does not come off—if the distribution is largely by cables supplying 500 k.w. or less—then the losses will be disastrous. I fear he is quite right, and can assure him that they become very serious indeed even with such low power-factors as 0·025.

He puts the "other transmission losses" at 5 to 7 per cent. For my part I should be very agreeably surprised if the other transmission and transformation losses come out at less than 20 per cent.

Mr. Cruise's table is both useful and interesting.

Mr. Baillie makes the useful suggestion that cable makers should endeavour to take the energy loss by observing the rapidity of polarisation, and points out that they have all the facilities for doing this in making their insulation tests. Mr. Baillie points out that Lombardi's power-factor of 0·068 was got on a cable and not on a condenser. It was a gutta-percha cable—the only one referred to in this discussion.

Mr. Whalley is quite right. My attention was first called to this capacity matter by the study at St. Petersburg of some effects occurring

Mr. Mordey. in the enormous network of Helsby cables there. He points out that the great capacity of 88 mfd. in that installation is a great advantage, as it balances the large wattless current of the transformers. I both agree and disagree. I learnt several things at St. Petersburg, and one was that the transformers supplied by the continental makers are much below our English standard. I was very much surprised to find transformers with 0·5 to 0·6 power-factor and with energy losses far greater than would be tolerated here. I do not agree that it should be necessary to provide a balance for such transformers, but, having them, it is no doubt also a good thing that the cables should have a large capacity. Mr. Whalley's interesting curves show how greatly the oscillograph must aid in the study of this subject.

Mr. L. Andrews is very much to the point in showing that the charging of his mains is costing him between £300 and £400 a year. He has gone so far as to rearrange his system to avoid this loss during light-load hours. I hope he will not forget to let us have the result of the comparative tests he intends to make when he gets his arrangements completed.

Conclusion.—It would require a great deal of time and space to do justice to the discussion. I have only been able to touch on some of the salient points. I am greatly obliged to all who have contributed to the elucidation of this subject. A mass of very useful additional and critical matter has been provided.

Attention has been directed chiefly to the loss of energy, and on the whole with a very useful result.

One object of my paper was to show that in the dielectric of cables losses were being entirely overlooked which were of engineering importance. I went so far as to say these losses were often more important than the copper losses. I gave the results of certain tests which may or may not have been correct. Similar values have been got on other cables by other people—capable observers like Mr. Minshall and Mr. Duddell. My tests have been disputed, but it has been shown by Mr. Sparks, the engineer of the company working the cables, that even on the basis of the lowest results obtained by later tests, this dielectric loss consumes more than 8 per cent. of the energy transmitted by the cables in question—that is to say, actually more than the copper loss. Since the discussion I have had occasion to go into the details of a power transmission scheme, and I find, so far from the dielectric losses being insignificant and unworthy of consideration, that on the basis of the lowest and latest values obtained by my critics, the dielectric losses will amount to considerably more than the copper losses. My study of this matter shows its very great and serious importance, especially in connection with "extra-high-pressure" schemes of transmission. In my paper I may or may not have been right as to the amount of the loss—that is not the essential point; the practical question is whether the loss is one which will affect practical engineering problems. I find that even on the most favourable assumption the loss is sufficiently serious to affect the way we must regard such problems in high-tension work as the choice of pressure, of frequency, of number of phases, of type, material, size, and construction of cables, of amount of load per cable

In more than one of these things this loss will be found to be the controlling factor. Mr. Mordey.

The PRESIDENT: We have already thanked Mr. Mordey for his paper, but may I ask you again to give your thanks to him for his contribution? The President.

The motion was carried with acclamation.

The PRESIDENT announced that the scrutineers reported the following candidates had been duly elected:—

Members:

Arthur Jacob.
Major-General Beresford Lovett, C.B., C.S.I., R.E.

Associate Members:

Stuart Bell.
Albert Campbell, B.A.
Alfred George Cooper.
Frank Little.
Edward John Neachell.
Charles Frederick Smith.
Albert Thomas Turney.

Associates:

Ernest Edward Allen.
Edward Harold Atkinson.
Alfred H. Cahen, B.Sc.
James L. Chambers.
Oliver Hammond Ellis.
Harold William Firth.
Charles William Forster.
Archibald Ernest Grant.
Charles Harold Higgins.
Frederick Walter Shorrocks.
Shelley Albert Stammwitz.
Charles Stewart, B.Sc.
Herbert Osborn Wraith.

Students:

Benjamin Baily.
Herbert Sugden Binns.
Joseph John Fasola.
Fred. E. Green.
Frank Clements Knowles.
John Ormiston McLaren.
Reginald Phillips.
Edgar Lloyd Smith.
George Edward Smith.
William Henry Taylor.
Sidney Mark George Teal.
R. Elliott S. Turnbull.
James W. Wilson.

Note.—The Institution is indebted to the *Electrician* for the blocks of the Figures on pp. 389, 426, 427, 455, 456, and 457, and to the *Electrical Review* for those of the Figures on pp. 460, 461, in the discussion on Mr. Mordey's Paper.—ED.

The Three Hundred and Fifty-Eighth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 21st, 1901—Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 14th, 1901, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that the list should be suspended in the library.

The following transfers were announced as having been approved by the Council, viz. :—

From the class of Associates to that of Members—

Edward Stanley Franklin.

From the class of Associates to that of Associate Members—

George Henry Corringham.
Frederic E. Nosworthy.

From the class of Students to that of Associates—

William James Cooper.
Edward Henderson Freeman.
Mendel Finkelstein.
Eustace Graham Sheppard.
Henry Sinclair Watson.

Messrs. H. J. Moysey and H. J. Hodges were appointed scrutineers of the ballot for the election of new members.

Donations to the Building Fund were announced as having been received since the last meeting from:—Messrs. J. R. Andrew, R. C. Barker, B. Balaji, R. J. Brown, H. H. Crockford, and C. W. Fourness, to all of whom the thanks of the meeting were duly accorded.

The PRESIDENT: The following resolution from the Newcastle Local Section has been received since our meeting last week :—

NEWCASTLE LOCAL SECTION. Resolution passed at the meeting of February 11th.

"That we, the Newcastle Local Section of the Institution of Electrical Engineers, express our grief at the death of our late Beloved Sovereign, Her Majesty, Queen Victoria, and our most loyal sympathy with our present Gracious Sovereign, King Edward the VII., and to request the Secretary of the Institution to forward the same to the proper quarter."

THE ELECTRICAL POWER BILLS OF 1900: BEFORE AND AFTER.

By WM. L. MADGEN, Member.

The difficulty of selecting a title to describe suitably a statement dealing with many considerations is a familiar one, and as I should wish to deflect discussion to more important questions, I will by way of preamble more particularly describe the scope and intention of this paper.

The intention, then, is to consider those conditions of our electrical industry which led up to the Electrical Power Bills of the past session, to describe briefly some of the main features of the Bills themselves, and to urge the Institution to determine what its attitude should be in face of the charges of backwardness continually being made in regard to the applications of electrical energy in this country.

To present a connected case it may be necessary to traverse a certain amount of ground well known to many of us; but it is in the hope that we shall be more firmly united in our struggle against the continuing legislative and other difficulties by which our path is sorely beset, that I ask you to review the situation this evening.

The new century has been acclaimed on all hands as the age of electricity, but every article on the subject in the magazines and daily press, and they have been many, appears to bewail the backwardness of this country in electrical enterprise.

The *Daily News*, in a leading article devoted to the recent visit of Mr. C. T. Yerkes, the American tramway magnate, suggests that now Mr. Yerkes is about to have his hand in on this side he might proceed to think out some magnificent scheme for enabling the people of

England, among other things, to work their factories and workshops by electricity !

The *Daily Graphic*, in an illustrated article describing electrical developments in northern Italy, notes that ". . . in one enormous industry there can be no doubt that we are far behind nearly every important country in the world. That industry is, in broad terms, the adaptation of electrical energy to the needs of man."

The *Daily Mail*, in a leader headed "Our Start in the Electrical Age," says : "In the twentieth century we find that our competitors are ahead of us in the utilisation and application of electricity. It is not to England that the foreigner comes for dynamos or electric tools or electric lifts or electric railway and tramway fittings. . . . Our streets are rarely lighted by electricity. As a motive power on our railways and tramways this new force has been as yet only occasionally employed, and the two or three electric railways of London are still a source of wonder to the inhabitants of our islands. All this is greatly changed from the day when we led the world. And whereas in the past we built the foreigners' steam railways, to-day the American is building, or proposing to build, our electric lines."

This article goes on to say that "it would be interesting to investigate the causes of our backwardness." The matter is, however, something more than "interesting," it is one that demands our most active concern.

Some of the indictments are more sweeping. It appears we are not all alone unhappy ; the *Pall Mall* considers that "in every department of public life our methods are out of date. We feast on words and trade upon tradition. . . . We forget that, though the camel may exist for a period upon its own hump, the process cannot last for ever. We have been like Brahmins in our pride and Mandarins in our methods, and that is a bad combination." *Fielden's Magazine*, the militantly British, finds space for Sir W. H. Preece to say, *à propos* of the dangers of foreign competition, that "we are a foolish and conceited nation, and blind to our own deficiencies."

The above are a few examples of the reproach which has ...me familiar to most of us. It may be the habit of the ...n, "in the intervals of blowing our own trumpets, to

rush to the other extreme, and to needlessly belittle ourselves." It may be that "prophets of ill are for ever telling us of the decadence of our industries and of the rapid progress of our rivals," but there is no comfort for the electrical engineer in any such commonplace.

After the events of the past year there is little need to urge that the national sentiment is as sound among members of this Institution as in any part of the community, and the conclusion which has been forced upon us from all sides that the extent of our department of industrial work is far behind that of nearly every other important nation in the world, is sufficiently mortifying.

There may be some of us who find amusement in the comments of the daily papers on technical subjects, but it should be remembered that the articles we scoff at are read by thousands of those of our fellow-countrymen who make up those sentiments and influences which affect us seriously, and it seems full time that steps should be taken to place the responsibility for our backwardness where it belongs.

We shall have to take our own part, or we shall find the public ordering German plant with the same complacency as they purchase German pianos.

It has become the custom with some of us to devote occasionally a few weeks to the visiting of other countries to benefit by the experience gained there in the developments of electrical work, and thanks principally to the energy of our Secretary this practice has become organised, and a largely increasing number now have opportunities of taking part in these expeditions.

The reflections ensuing upon these visits have no doubt varied in depth, and in character, with the individual and the nature of his occupations.

One of our greatest science teachers, in his Presidential address which lives in our memory, referred in inspiring terms to the visit to Switzerland and the revelation it was to us. We were, he said, very much like what engineers of 1870 would have been if brought suddenly into a generating station, and attributed much, if not most, of our backwardness to our knowing too little theory.

I am inclined to the view that this lack of theoretical knowledge is an effect rather than a cause of our troubles. In Germany and Switzerland particularly we see a systematic

devotion to technical and scientific training, but the Americans have applied themselves rather less to the principles than to the applications of science, and it is the latter country which is probably the most in advance of us in electrical engineering.

It is unnecessary, however, to give occasion for a discussion on theory *versus* practice. I am sure we take it to heart that we have too little theory, and will loyally support those who are endeavouring to apply more scientific methods to our manufactures. I am more apprehensive just now concerning the scope for the employment of the young electrical engineer when he has been prepared by the most approved methods.

We have no feeling but cordial goodwill for those friends abroad who have made our visits so pleasant and instructive; but the reflections of many of us on our return have been those of indignation at the obstacles set in the path of our industry by the governing bodies of this country, and of resentment at the wretched waste of energy and enterprise which they have occasioned.

The electrical engineers of the United Kingdom are not to blame, and there is no occasion for any apology on their behalf. I am aware there may be those who are interested in the maintenance of old methods of working, and we have internal complaints now and again for which the Observatory at Kew may for the moment be taken as a symbol. But we can put such issues past us for the present, and consider whether it is not our duty to make a united effort to secure liberal measures of enfranchisement.

The position cannot be attributed to lack of ability or inventiveness on the part of British engineers. Some of the most important improvements have been worked out in this country, but our environment has been too much for them, and they have found their fullest practical application abroad.

One of the first electric tramways on a practical scale was put down in the north of Ireland by Lord Kelvin, Mr. Traill, and others, but the effect of the Act of 1870 which had been humorously entitled "An Act to facilitate the construction and to regulate the working of Tramways," been disastrous to tramway enterprise in this country.

he principles of dynamo construction were worked out

at an early stage by the late Dr. John Hopkinson, whose share in the evolution of the three-wire system is also well known to us.

To Swan as much as to any other the world is indebted for the incandescent lamp.

In the early eighties, work of the utmost value was done by Ferranti and others on the details of alternating-current transmission.

It is unnecessary for me here to dwell upon the abundant evidence in support of this. It is registered for the most part in the Journal of the Institution, and much of it is recorded in the work of others on every continent.

What is the reason, then, that our electrical industry is behind that of nearly every important country in the world?

It is due in the first place to silly legislation by Parliament and to obstruction by the numerous local authorities entrusted with arbitrary powers. In the second place it is due to a class of quasi-officials and their associates to whose direct monetary advantage it is that an opposition should be entered to every project in which they are not employed.

There may be contributory causes, but to these, and others that flow from them, our chief difficulties may be assigned.

Ere this Institution was founded an obstacle had been prepared for us. In 1870 was passed the Tramways Act to which I have referred, and these are some of its leading provisions. A tramway cannot be authorised by Provisional Order without the consent of the local authority of the district. If the proposed tramway is to run through two or more districts, and consents have been obtained in respect of two-thirds of the length, the Board of Trade may upon inquiry dispense with consents for the remainder.

When the procedure is by Provisional Order, the construction of the line can be absolutely blocked by notice given in the prescribed manner by one-third of the owners, or of the occupiers, of the premises abutting upon the road where for so short a distance as 30 feet or upwards there would be less than 9 ft. 6 in. between the outside of the footpath on either side of the road and the nearest rail of the tramway.

The most onerous condition is provided in the notorious

section 43 by virtue of which the local authority can purchase a tramway at the expiration of twenty-one years from the date of the Order, or of any subsequent period of seven years, on the terms of paying the then value of the tramway, exclusive of any allowance for past or future profits of the undertaking, or any compensation for compulsory sale, or other consideration whatsoever.

We shall agree with Mr. Granville C. Cunningham, general manager of the Central London Railway, that "there is perhaps nothing that has done so much to prevent the relief of overcrowding and congestion as the Tramways Act of 1870. Its provisions in rigidly limiting the term of the concession to twenty-one years, and practically fixing the price at which the Municipality may take over the undertaking at something far below its value, have effectually checked the growth of electric systems."

If procedure by Special Act is resorted to, the Standing Orders of the two Houses require in like manner the consent of the local authority, so that the undertaking is not furthered in that respect and is equally liable to be bled for the purpose. The frontagers are given a locus to oppose, so that a small number of them are not so well able, through caprice or other motive, to prevent the construction of an important line; but the expense and risk of procedure by Special Act have been sufficiently serious deterrents.

I think the nett results will justify us in agreeing with Mr. Balfour Brown that this same Tramways Act was "a very silly contrivance indeed."

Consider the waste of energy and of enterprise in battling against such conditions, and whether it is any wonder our countrymen have to complain that we are lamentably behind the rest of the world in the cheap and rapid transport facilities afforded by electric traction.

We have [1] pleaded that there is no country in the world which would benefit more than our own by the provision of comprehensive electric tramway systems, or that stands more in need of them to relieve overcrowding, yet their introduction has been slow and halting, and carried out in the face of bitter opposition. Instead of lending a helping

[1] See particularly paper by Mr. Granville C. Cunningham, in the *Tramway and Railway World*, December, 1889.

hand, governing bodies seem to have thought they were doing the public a service by narrowing enterprise down to the scantiest possible outlets, and in some cases by establishing an effective blockade against it.

Twenty-six years elapsed—near upon a generation—and it was not until 1895 that the repose of the Board of Trade was disturbed—a body curiously constituted and still more curiously named—and it was found that something really must be done. A Commission was then appointed to consider the question of Tramways and Light Railways, and as a result of their report the Light Railways Act was passed in 1896, only four years ago. But this was only a tentative provision, it expires this year, and unless some measure of enfranchisement is passed, we shall be thrown back upon the Act of 1870, which remains effective upon the Statute Book.

The other great department of our domain, the general supply of electrical energy for industrial and domestic purposes, is even yet almost entirely governed by the Act of 1882, which was passed at a time when "the state of the art" was such that electrical energy could only be supplied economically for a distance of a mile to a mile and a half from the generating station.

In effect the legislation was made to match the fact that at least one separate station was then required for each town, and the Town Council was constituted the authority which could either undertake the work itself, or consent to the acquirement of the necessary powers by a company.

In either case the Act contemplated the local authority being the ultimate owner of the local undertaking. The clauses as to consent, the period and the terms of purchase, were inspired by the spirit of the Tramways Act of 1870, and the effect upon electrical engineering as a national industry has been equally disastrous. The result was a fiasco. What did Parliament then do? Nothing until 1888, when it climbed down in a half-hearted way—analogous to its performance in connection with the Light Railways Act—and passed another measure extending the purchase period from twenty-one to forty-two years, but leaving the onerous consent clause and the ridiculous terms of purchase as they were.

Until after the Act of 1888 we could do practically

nothing in the way of establishing electrical supply undertakings.

Circumstances were more favourable in America and Germany during those six valuable years, and, encouraged by the home demand, which is an essential condition of enterprise abroad, the manufacturing trades of those countries laid the foundations of their great export business in electrical plant and accessories, the consequences of which we are feeling to-day.

America and Germany now hold the great bulk of the export trade of this character, not only to the countries of Europe, and to South America where this country has heavy financial interests, but also to our own Colonies and the United Kingdom itself.

You will remember that the whole of the steam engines, dynamos, electric lifts, etc., for the Central London Railway, were supplied from America, and many other examples will occur to you.

As I have said, the legislation was in effect made to match the fact that in 1882 it was necessary to put down at least one generating station for each town, and the conception of the situation by many local authorities is shown by their persistent endeavours to wall themselves in, so to speak, against the improvements by means of which the area of economic supply has vastly outgrown such limits.

The Acts of 1882–88 and their administration have proved entirely congenial to the narrow exclusiveness of these bodies, who are endeavouring even now in their opposition to the Power Bills, to insure themselves against the developments of science and a cheap supply rather than impair the prospect of their being able to have an isolated municipal plant all to themselves some day or other.

The statistics available afford us the following figures:—

	Electricity Works in operation.		Undertakings for which Orders have been obtained, but Works not yet carried out.	
	Local Authority.	Company.	Local Authority.	Company.
1901	130	68	212	55

Of the fifty-five undertakings which have not yet been carried out by companies, only three of the Provisional Orders (not taken by transfer from the local authority) are two years old yet, and thirty-two of the remainder only date from last session. Many of the works are now in course of construction, and very few are being hung up in any way that I am aware of.

Of the 212 Orders not yet carried out by local authorities, the works in eighty-nine cases are, I believe, in course of construction or have been more or less decided upon, leaving a balance undecided of no less than 123.

Under a Provisional Order, the time within which a supply should be available is two years from the date of the Order, but of the 212 towns referred to a large proportion have exceeded that period, some of the dates tailing back so far as 1891, 1892, and 1893.

The figures do not really show the full extent to which business is retarded, because no one has tabulated the local authorities who not only have not applied for a Provisional Order, but of whom it is known that they would have blocked any application; but the state of affairs is sufficiently shown by the fact that throughout the whole of the United Kingdom electricity supply under the Acts is only available to the public in some 250 districts.

Now we know that the consumer can procure his electrical energy on a more favourable basis for all concerned from an undertaking dealing with a comprehensive area and varied classes of demand, than he can get it from a relatively small local station. This being the case, it may be supposed that it could only be a question of time when the obstacles, obstinate though they have been, to the cheap supply and to a large extension of the industry must give way somewhere.

The first break in the clouds was observed in 1898, when a Joint Select Committee of the two Houses was appointed (at the instance of the Lords) to consider and report upon a reference in regard to "Electrical Energy—Generating Stations and Supply."

Time does not serve to detail the circumstances which led up to the appointment of this Committee, to describe the Bills then pending in Parliament in which it was proposed to give effect to the developments of electrical science,

nor can I read you the full terms of the reference or of the report,[1] but I extract the following paragraphs from the latter :—

"Where sufficient public advantage is shown, powers may be given for the supply of electrical energy over an area including districts of numerous local authorities, and involving plant of exceptional dimensions and high voltage. The Committee further think that undertakings of this character may properly be authorised on conditions differing in some respects from those imposed by and under the existing Acts.

"The Committee consider that the provisions of the Electric Lighting Act, 1888, which require the consent of the local authority as a condition precedent to the granting of a Provisional Order, should be amended. In their opinion the local authority should be entitled to be heard before the Board of Trade, but should not have, so to speak, a provisional veto, only to be dispensed with in special cases by the Board of Trade."

It was the feeling of electrical engineers that the report was too moderate considering the attitude maintained by the local authorities and their notorious misuse of the powers conferred upon them by the Acts, but this feeling is turning to one of dismay at finding that during the two or three years which have ensued nothing whatever has been done to carry out the recommendation last mentioned—which is a full justification of much that I have urged—and that their exertions continue to be very largely wasted.

It might be gathered from the continued lamentations of the press that the electrical engineers of this country were likely to be put upon their defence, and this might indeed be a consistent involution of the legislation which has already tried us very severely in another sense, but it cannot

[1] In the House of Commons, July, 1898, Mr. Ritchie, replying to Mr. Kimber, said that the report of the Joint Committee would be carefully considered by the Board of Trade, but legislation would be required to give effect to some of the Committee's recommendations, and he was afraid that the prospect of passing a Bill through that session was very small. The prospect must have been small indeed, for the process of official consideration has extended not only over 1898, but also beyond 1899 and 1900.

be said that we have been slow to act upon any measure of encouragement available to us. This was true of the Act of 1888, so far as its provisions would allow, and there has been absolutely no hesitation or delay in turning to practical account the paragraph in the report from which I have just read, with reference to the supply of electrical energy over extensive areas, by means of plant of exceptional dimensions and high voltage. I have referred to the Bills pending in Parliament at the time of the report, and we must acknowledge the valuable pioneer work of some of our members in connection with them.

The Bill which affected our prospects most strongly, although it did not pass, was promoted by the General Power Distributing Company in 1898–99, and was familiarly known as the Warsop scheme, the project being to distribute electrical energy over an area comprised within a radius of 26 miles from Warsop in Nottinghamshire. This district includes such populous centres as Sheffield, Rotherham, Nottingham, Lincoln, Doncaster, Derby, and Chesterfield. The powers sought were to lay trunk mains throughout the area, to give a supply of electrical energy except where the local authority was itself empowered to supply under an order or Act and agreed to take a supply in bulk from the Company on arbitration terms. To supply direct in all cases to consumers taking 10,000 units per annum and upwards. The conditions in regard to district, etc., were favourable, and large quantities of coal slack were available at 2s. per ton in the neighbourhood of the proposed power-station. The Bill, as we know, did not then pass, but the powers which it sought to obtain have a considerable interest from the point of view of to-day, as in varying degrees they are reflected in the four Power Bills which were passed last session.

Before summarising the considerations relating to the supply of electrical energy over extensive areas, it will be convenient to follow the course of events. Public opinion became gradually informed on the subject, and it is to be hoped a little moved at the sense of national backwardness, and in the session of 1900 four Electric Power Bills, each for supply over important English areas, passed through Parliament. These were :—

THE COUNTY OF DURHAM ELECTRIC POWER SUPPLY.—This area, about 250 square miles in extent, comprises the main portion of the Durham coal-fields and one of the leading manufacturing and shipbuilding districts of the north-east coast. Provisional Orders had been obtained authorising the retail supply to consumers in the chief towns, viz., Gateshead, Jarrow, and Durham City. The British Electric Traction Company had undertaken an extensive system of electric tramways in Gateshead and district, and have since obtained powers for lines in the Jarrow and Durham City districts. The Power Act authorises the laying of trunk mains throughout the area, and the supply of electrical energy in bulk to undertakers authorised to supply, and also to undertakers authorised to use it for prescribed purposes.

Thus the supply may be given at once for general use in Gateshead, Jarrow, and Durham City, and for electric tramways and light railways in Gateshead, Jarrow, and other parts of the county.

The first portion of the main power-station on the river Tyne at Gateshead is rapidly approaching completion, and will be available this spring for a comprehensive system of supply which is being prepared in readiness for it.

THE NORTH METROPOLITAN ELECTRIC POWER SUPPLY.—This area, about 325 square miles, includes the great suburbs to the north of London, from Tottenham on the east to Harrow on the west, and the growing manufacturing districts along the river Lea. It covers the area within which the extensive North Metropolitan electric light railway has been carried through by interests friendly to those of the Act. The provisions and general considerations are the same as in the County of Durham Bill, and the same general policy was followed.

THE LANCASHIRE ELECTRIC POWER.—This Act takes in the whole of Lancashire south of the river Ribble (except Manchester, Salford, Bootle, and Stockport), an area of about 1,000 square miles. The district may appear somewhat large, but a great part of it is undoubtedly very suitable, since it comprises a large number of collieries, engineering works, cotton mills, and a variety of other industries. The Act contains powers to lay trunk mains throughout the area, and to furnish electrical energy in

bulk to undertakers authorised to supply, the promoters having relied upon the cheapness of production at large generating stations as sufficient to secure holders of Electric Lighting Provisional Orders as customers for bulk supply.

THE SOUTH WALES ELECTRIC POWER DISTRIBUTION.—This includes the whole of the County of Glamorgan and extends into Monmouth as far as the river Usk (also including Newport), an area of about 1,050 square miles. The principal towns are Cardiff, Swansea, Newport, Barry, Merthyr, Pontypridd, and Neath, and the district thus comprises the great colliery, shipping, and manufacturing districts of South Wales. The provisions of this Act are similar to those of the Lancashire Act, with one important difference. The South Wales Company were given powers to supply direct to *any* person for power purposes, and for lighting any premises on some part of which the power is utilised, provided only that in a local area where an Electric Lighting Provisional Order exists, the consent of the authorised distributor in such area is first obtained. If such consent is withheld the Board of Trade may dispense with it, if in the opinion of that body the authorised distributor under the Provisional Order is not willing and in a position to give the requisite supply to the power-user upon reasonable terms and within a reasonable time.

All these Acts contain a sliding-scale clause as regards prices and dividends, also powers for the revision of the scale every ten years by the Board of Trade, 8 per cent. being taken as the normal maximum dividend. The general clauses follow pretty closely the usual electric supply practice; for details reference should be made to the Acts themselves.

There are a few examples of groups of Provisional Orders having been obtained for adjoining districts by arduous negociations extending over several sessions. These undertakings typify many of the difficulties with which electrical engineers have had to contend, but they are not more fully referred to here as the form of procedure was not by Power Bill. There is, however, an important Northumbrian undertaking dealing with the north bank of the Tyne from Newcastle to North Shields (excluding the latter) with part of the hinterland

which belongs to both classes and should not be omitted from the list, especially as it has been one of the first to get to work.

The main purpose of the Power Acts is to keep the number of power-stations within economical limits, and by the selection of suitable sites and the equipment of works of considerable magnitude to enable electrical energy to be transmitted in such a manner that the retail price to the consumer will be reduced to a figure which will compare with, and in many cases be far lower than, that of any other form of power, whether gas, steam, oil, or other agent.

The enormous development of electricity in the United States and Canada and on the Continent of Europe, and the numerous great electrical power distributions over large areas, in those countries are in themselves practical evidence in favour of the principles we are advocating. It cannot be urged that the requirements of this country, so far as trade and cheap production are concerned, are different to those obtaining in the countries mentioned.

The absence of undertakings of the kind in the United Kingdom has not been on account of any difficulty or impossibility on the engineering side, or from lack of suitable conditions. We have few large water powers it is true, but there is an equally good source of power available, and coal in this country can replace, on favourable terms, the water power available elsewhere for the generation of electrical energy. Moreover, even in those parts of the country where coal is more expensive than in the coal districts themselves, relatively cheap electricity may be available if it is generated in sufficient quantity at large power-stations, and the supply from such stations is distributed over a suitable area. Capital charges, management, rent, rates, and taxes usually form a larger proportion of power-station costs than fuel, but the greater the importance we have to attach to the fuel item, the more necessary it is to adopt the most comprehensive methods and to concentrate and economise its use.

Under the Electric Lighting Acts, and the conditions heretofore existing, the scarcity of the electricity supply in this country has been due to the high cost of production. Even in the more developed areas the cost has been too

high as a general rule to admit of its being freely used by the consumer on terms more advantageous than those upon which he can employ steam or gas for industrial purposes, or gas or oil for domestic service.

Statistics have shown us that the average cost of production and supply to consumers becomes lower as the output of the power-station increases, but the difference between stations supplying one million and four million units per annum is less, and not in proportion to that obtaining between smaller stations with much less difference of output. Without an increasing "diversity factor" this difference would tend to disappear as stations increased in size.

A good "diversity factor" can only be achieved by combining with electric lighting the supply of energy for as many and as various other purposes as possible, and, so far as lighting is concerned, the supply to every class of consumer. As the area of supply is extended, the "diversity factor" tends to improve owing to the difference in the incidence of the demand in different districts.

I shall not attempt to follow the more technical aspects of the subject just now, as they afford scope for many papers, and certainly for more discussion than you can give to them this evening; but it may be well to summarise the points for and against the old system and the new, *i.e.*, the supply from small local stations and the supply from main power-stations :—

Supply over comprehensive areas from main power-stations in selected positions.

ADVANTAGES :

1. Comparatively large field for development.

2. High load-factor obtainable, all the plant being used to the best advantage.

3. Cost of fuel and handling can be reduced to a minimum, as the power-station can be located where fuel is cheapest, fuel handling most economical, and water is available for condensing.

4. Low running costs, management expenses and maintenance per unit sold, as the result of a very large and regular output.

5. The low cost of plant per kilowatt installed, and the increased economy in running with very large sets.

6. Low rents, rates and taxes; the difference between town and country.

7. Economical provisions for extensions to plant and buildings.

8. Low costs and charges for electrical energy for all purposes possible in consequence of above advantages.

9. Removal of the power-station with its chimneys, etc., outside the residential district.

DISADVANTAGES from point of view of Local Authority :

1. The transmission mains must pass through their area whether supply be taken or not.

2. Sentimental preference for complete independent plant of their own.

Supply in small local areas from separate stations.

ADVANTAGES :

DISADVANTAGES :

1. Small field for development.

2. Relatively low load-factor.

3. Cost of fuel, handling it, and water supply depend on immediate local conditions, favourable or unfavourable.

4. High running costs, management expenses, and maintenance, the costs per unit sold being generally higher the smaller the station.

5. High relative cost of machinery per kilowatt installed, and lower economy in running.

6. Rents, rates and taxes relatively higher.

7. The extension of buildings is frequently very expensive, owing to disturbance and other difficulties incidental to town sites.

8. High charges for electrical energy for all purposes as result of these disadvantages.

The story of George Stephenson and the cow on the line has come down to us as typical of the prejudice and the ignorance with which[1] railways had to contend in their early days; and so, too, when the early history of the electrical industry comes to be written, the part played by the local authorities in their strenuous opposition to the Power Bills will be a record of reproach to them.

For the purposes of this opposition there was a conference of local authorities in Manchester in January, and a meeting of the Association of Municipal Corporations in May. The object of the first meeting was to prevent the second reading, and that of the second to influence the decision of the Parliamentary Committee.

The methods adopted by those who endeavoured to wreck the Power Bills were strongly condemned in the course of the second reading[2] debate, and the President of the Board of Trade found it necessary to repudiate a garbled report which had been circulated as to his remarks upon the Bill for the Warsop project in the previous session.

Every one interested in the welfare of the industry should read and think over that debate. True, the Bills were read a second time and committed, but what a curious light is thrown by the discussion upon the difficulties with which we have to contend!

Perhaps there is time to mention two examples. Mr. Ritchie said, ". . . I hope the House will give its attention to the very important considerations in this case before they decide to reject on Second Reading a Bill that is fraught with so many possibilities. It is true, I think, that the electrical enterprise of this country is in an exceedingly backward condition; it is inferior with regard to light, and certainly with regard to the conveyance of power, to many European countries, and it is greatly inferior to North America and Canada. It may almost be said that there are

[1] In 1801 we had no railways in the sense we now use the term. To-day the railways of the United Kingdom extend to about 22,000 miles of line, constructed at a cost of about 1,300 millions of pounds. The annual gross receipts now exceed 100 millions, and of the expenditure, which amounts to over 60 millions, fully one-half is distributed in wages to over half a million employés.

[2] See *Parliamentary Debates*, No. 2, vol. 79 (page 1,374 and following), published by Wyman and Sons, Limited, Fetter Lane. Price, 1s. 3d.

villages in North America which are in possession of advantages in connection with electricity which some of our largest towns do not possess. It cannot be doubted that there is a great demand for something to be done. At present electric light matters are governed largely by the legislation of 1882, and it has been said that this Bill is largely in opposition to many of the enactments in the Act of 1882. If no other charge or argument could be brought against this proposal, the argument of the opponents to this Bill would indeed be weak. It must be remembered that it was the Act of 1882 which more than anything else had delayed and hampered the development of electrical supply, and in so far as this Bill departs from that Act, I think its departure is amply justified by the condition of things at present existing in the electrical world."

Here we have a member of one of the strongest Governments of modern times, the Minister entrusted with legislation affecting the trades of the country, who has realised the extent and cause of our backward condition in relation to a great industry, and ingenuously confessing that during five long years of office one of the main causes of the trouble has remained effective upon the Statute Book.

Sir William Harcourt said, ". . . I do not altogether share my hon.[1] friend's objections to great enterprises being carried on through private sources. That was a question which occupied fifty or sixty years ago the attention of this country, and that was at the time of the commencement of the great railway interest. That question was decided by the wisdom of the great statesman Sir Robert Peel. We know that Sir Robert Peel was much attacked at that time for throwing the railway enterprise of this country into private hands, and not adopting the system so largely followed on the Continent. I look forward to this question of electricity and electric supply as the great question of the future, and it is from that point of view that I wish to refer to the subject. If this company is prepared upon proper conditions to supply electricity to any part of the country, I am not opposed to that. No man can say to-day what part electricity may not play in the industry of the country, and that is a point which the House of Commons should keep

[1] Mr. Broadhurst.

in view. But what are the conditions which ought to be imposed? What was the policy which was pursued with regard to the railway companies? Parliament did not leave it altogether to particular promoters of Bills; Parliament did not leave it to the discretion of individual committees. They placed the whole of that great enterprise, upon which more than a thousand millions of private money has been expended, greatly to the benefit of the country—a sum larger than the National Debt, and now paying interest at least of 4 per cent., and one of the greatest investments for the savings of the country—under general legislation. I think that a model which we ought to follow in this instance. But what was the method which Parliament in those days adopted in dealing with the railways? They did not allow particular promoters to take their chance in individual committees. They placed the whole of that great enterprise, as I have said, under general legislation. . . ."

This statement of Sir William Harcourt goes to justify the charge that the Legislature has neglected an industry the importance of which he describes in suitable terms, and it also leads us to the economic aspect of the question. Comparisons have been drawn between the benefits first derived by every class of our community from the applications of steam power and of railways, and those which have accrued to other nations in larger measure than to ourselves from the uses of electrical energy. The United Kingdom itself has not yet lost any material part of its natural advantages for the manufacture of engineering material, or of scope for their employment. In what way has destructive legislation acted so as to place us in the position of inferiority we are reproached with to-day?

It has, among other things, tended to destroy cumulative investment effect. Savings out of the profits of a business tend to go back, as an additional investment into that business or some department of trade allied with it. Part of the profits derived from railway enterprise undoubtedly went in again, and attracting new capital to it, provided means for building new lines and for equipping rolling mills, foundries, engine works, and other undertakings which have provided employment for thousands of our fellow-countrymen. No influence has done so much

during the past hundred years to stimulate enterprise, to encourage commerce, and to develop the resources of any country.

In our own time legislation has not only deprived the great mass of the people of the direct benefits of electrical science, but it has made much of what little has been done indistinguishable to the investor from local government loans for drainage, refuse destructors, slaughter-houses, and other purposes most necessary in themselves, but somewhat in the back-yard of civilisation.

It may be replied that Sunderland, for instance, has just declared a profit out of its municipal tramways, but what advantage has this been to any one? The fares have been substantially the same as would have been charged by private enterprise or the amount would not have been earned. The local rates we may be sure will not go down, and if they are a trifle lower than they otherwise would have been, those to benefit most will be the railway company and other large ratepayers who have contributed least to the tramway revenue.

The banks, insurance companies, and such institutions which provide much of the local government loan capital will get their 3½ per cent., and part of it may be re-invested in colourless loans elsewhere, but of that great encouraging influence towards the growth of healthy industry which I have imperfectly described as the cumulative investment effect, there will be little or nothing.

And the money for the purpose has been deflected elsewhere. The subject is a complicated one, and it may not be in place to follow it here, but it is a significant fact that *exclusive* of foreign loans the yearly increase of capital from this country invested abroad averages at present about £30,000,000. One tendency of this has been to set more people to work abroad instead of at home, and to increase the competition against home industries.

In the interests of which class of the community the enactments I have referred to were passed and have been administered by the various authorities, it is difficult to say. They have not benefited the general public, the complaint made on their behalf is that they are debarred from the advantages of electricity; and least of all have they benefited the working man, who finds that while the electric

light and comprehensive electric tramways are not for him, hundreds of thousands of pounds' worth of foreign-made plant and accessories are landed on our shores.

The working classes have suffered in another and, perhaps, a more serious way from the division of the country under innumerable local authorities endowed with powers such as I have described. The more or less arbitrary boundaries of these authorities derive in some cases from the middle ages, they are not and cannot be adapted to one and all the various means by which science and enterprise can be brought to the aid of the general community, and it can be shown that in practice the system tends to aggravate some of the grievous social and industrial problems of our own times.

These authorities number among them men of great ability and benevolence, but their collective action is frequently controlled by traders, property owners, and others who act upon the view that the best interests of their several districts lie in the direction of increase of rateable value and of population. Add to this a large official class alive to the advantage of increasing the importance of its own environment, and there need be little wonder that each district shows a tendency to "cuddle up" all it can attract, and that there should be grave reason for our being urged "to get rid of that which is really a scandal to our civilisation, the suffering which many of the working classes have to undergo in order to obtain even the most moderate, the most pitiable accommodation."

There may be many fibres to the scandal of the housing of the poor, but the conditions most favourable to its growth are to be found in our system of local government and its administration.

The future of the electrical profession is so interwoven with social questions that we cannot escape their consideration. Mr. Balfour has said, "I believe that electrical traction is going to play a far larger part in the solution of this difficulty"—the housing of the working classes—"than any of the strange schemes I have analysed"; Mr. Lough that "It has been agreed by everybody that the chief means of improving housing accommodation is to spread out the city and destroy congestion, and it is agreed that there is no way of doing this effectually except by providing better facilities for traffic."

No one can question the advantages of improved traffic facilities, but if the direction morning and evening is to and from a congested trade centre the problem is only half solved. It is to electric power distribution on a sufficiently comprehensive scale to adapt the country districts to manufacturing purposes, in company with inter-urban connection by means of electric traction, that we must look for the greatest agency in ameliorating the conditions of the working classes in all their surroundings.

It can scarcely be asked what has all this to do with the Institution of Electrical Engineers, for we have seen that the community is conscious of the backwardness of our work, and faced by social phenomena such as those to which I have referred, we are called upon to perform our part in counteracting them. Electrical science is ripe for the occasion, and it therefore appears to be our duty and to our interest to convince the Legislature as to the means it should take to enable us to carry on the services assigned to us.

As some technical objection might possibly be raised to any action we may take in this direction, we shall find, on consulting the Memorandum and Articles of Association, which describe the scope and general organisation of the Institution, that (among allied objects) it was established "*To promote the general advancement of Electrical and Telegraphic Science and its applications, . . .*" (*Sect.* 3 *B*), and "*To do all such other lawful things as are incidental or conducive to the attainment of the above objects*" (*Sect.* 3 *D*).

Article 53 says that "*It shall be the duty of the Council to adopt all due means for the advancement of the Institution; to provide for properly conducting its business in all cases of emergency . . .*"; and a preceding *Article* 49 provides that " . . . *The Council may appoint Committees chosen from their own body, and Committees for special purposes consisting of Members of Council and Members, Associate Members, or Associates of the Institution and others, with such powers as the Council may prescribe.*"

Thus the terms of our Constitution not only authorise action being taken, but they also appear to intimate the course to be followed in dealing with any obstacles with which we may have to contend, and I trust that the

discussion will give the Council an indication as to the desirability of appointing a special committee, as provided by *Article* 49, to consider what steps should be taken to remove the restrictions upon us, some of the effects of which I have endeavoured to describe.

The President.

The PRESIDENT: We have received a telegram from Mr. Garcke, Mr. Stephen Sellon, and Mr. Morse, stating that they have been unavoidably detained in the country. The following letter has been received from Mr. Vesey-Knox:—

Mr. Vesey-Knox.

Mr. VESEY-KNOX (*communicated*): I am sorry not to be able to avail myself of your kind invitation to hear Mr. Madgen's paper, which I have read with interest. If I might venture on a word of criticism it would be this:—The question seems to me to be altogether one of price. In the case of tramways other considerations operate; but, so far as mere electrical supply is concerned, Parliament has now in principle decided that any company offering economic advantages shall be given an opportunity of supply, with due regard for vested interests. If the companies can, in fact, supply cheaply, they have now a wide field open to them. They can practically force local authorities whose Orders are hung up to take current by offering it at a cheap price and without capital expenditure. The real reason why many people who are not prejudiced in favour of socialistic experiment have supported municipalisation of electric light undertakings is that so many of the companies have charged such high prices for current. This has been a short-sighted policy, even from the point of view of the particular companies themselves, as, except in districts where people will have the best light at any price, the cost of supplying at a high price is much greater than that of supplying at a low price. With high prices you may have a big district and big works very irregularly employed, and the undertaking loaded by a cost in distribution mains out of all proportion to the number of consumers. But even more unfortunate has been the general effect upon private enterprise, by depriving promoters of the really practical argument against the obstruction of the less enlightened local authorities. It is in some ways a pity that the sliding scale clauses were not applied to electric lighting undertakings instead of the clauses giving the Board of Trade power to alter the maximum price after seven years (s. 31 of Electric Lighting (Clauses) Act 1899). The case of Cork, which was not a favourable field, is a remarkable proof of what low prices may do. There a supply at an average price of under 3d. was profitable in the first year. At 6d. probably no profit would have been made for five years.

The sliding scale has now been applied in the case of the Power Bills, and it is to be hoped that this will have the natural commercial effect upon the undertakings. I do not believe the public are disposed to look unfavourably upon even "monopolies" if they derive therefrom, in comparison with other districts, evident and palpable facilities; and, after all, it is the public who, in [illegible] control the local authorities.

Mr. Sellon.

Mr. R. PERCY SELLON: So much of the time of our Institution is taken up in the discussion of subjects of a purely scientific character, that it is seldom—and, for my part, I think too seldom—that we have before us a paper of the kind which Mr. Madgen has read, dealing with Electrical Engineering questions from their political and industrial aspects. This paper relates to principles rather than to details, and it goes down to the very foundation of the issues upon which the progress or backwardness of electrical engineering in this country depends, and upon which the great majority of the members of this Institution depend for their livelihood. In my opinion, therefore, the paper which Mr. Madgen has brought forward is one of great importance; and I think its interest falls under two heads, namely, the technical problems attaching to these large power-distribution schemes, and the consideration of the matter from the collectivist or political point of view.

With regard to the intrinsic merits of these schemes, Mr. Madgen has dealt with them at considerable length, and on page 490 he has stated fully, and, I think, fairly, the arguments for and against such power schemes, which depend upon this root question: Is it in the interests of the public as a whole that there should be a few large stations distributing over large areas, in preference to a larger number of small stations distributing over smaller areas? There are problems of generation and distribution which I do not propose to deal with. I will only say, in general terms, that it appears to me that the spirit of the time, which makes for the consolidation of industries and trades in all fields, points by analogy to the economic merits of large power-distributing schemes as compared with small stations located in each town. It seems to me that it is a reproach to our Institution that we should be palavering and debating over the question whether or not these schemes are possible, while countries which were far behind us in industrial progress half a century ago have already proved them to be desirable by such stations as those at Niagara and Messina, Rheinfelden in Germany, and Chevres in Switzerland, and Fiume in Italy. Why is it that we are at present in this backward state? I think the answer is, as Mr. Madgen has pointed out, because the progress of the electrical industry in this country has not been determined by its merits, but by political considerations. It is a misfortune for the electrical industry that its birth and its growth, up to the present, have been coincident with the birth and growth of the "municipalising" idea. Hence it has come about that our industry has become the plaything of politicians and of those who are anxious to municipalise all public supply in the interests of the democracy. There is no doubt, to my mind, that that is the real explanation of the fact that in this country we are quite without large distributing schemes, whilst abroad they are already in existence. Note the fact that this difficulty has arisen at every stage of the electrical industry. In the case of electric lighting, the Electric Lighting Acts retarded growth; then the Electric Tramways Act of 1870 was only with great difficulty amended by the passage of the Light Railways Acts, which late in time have given Electric Tramways a possibility of existence; and now these electric power schemes have been made the battle-ground of the two contending

parties—those in favour of municipalisation and those who advocate leaving the development of new industries in the hands of private enterprise. It is true that the Legislature has at last made a tardy recognition of its errors of judgment in the past, and by the passage of these Bills has recognised that the public interest does point to infant industries in their early stages being placed in the hands of private effort. But, I would like to point out that help has come almost too late. Investors, who, after all, supply the sinews by which these schemes are rendered possible, have grown to look askance at electrical enterprise. Parliament has bandied about electrical enterprise, has pursued such a vague policy, has imposed so many regulations upon electrical schemes, and local authorities have been so largely pandered to, that it is very small wonder that the investor can now with the very greatest difficulty be persuaded to believe that there is for him commercial advantage in embarking upon electrical enterprise. We often hear it said by politicians and others that we shall "muddle through somehow." Well, we may mismanage a war, we may have mismanaged many questions in the past, and the country has "muddled through" because those wars and those enterprises were conducted under conditions where this country had either the superior power of the purse behind it, or armaments, or some factor, which enabled it in the end to overwhelm its enemies. But in electrical enterprise that is not the case. Our great rivals in America, in Germany, in Switzerland, have all the advantages—material, financial, and technical—that we possess; and therefore the theory of leaving these things to "muddle themselves out," rather than handling them by concerted and associated action, is one that must be dismissed with regard to the electrical industry. Mr. Sellon.

What is the cure for this state of things? The roots of the trouble lie so deep in political and other considerations that we cannot in this Institution survey the whole field over which they are spread. The practical question for us is, What can this Institution do in its own sphere to add its quota towards the removal of the disabilities from which the industry is suffering? The answer to that lies in the suggestion that Mr. Madgen has thrown out. I know that we, as an Institution, are rightly on our guard against taking any action which may appear to benefit one class of our members at the expense of any other class. But where the interests of electrical engineering as a whole are at stake, I contend that this Institution should take a more active part in the support of those associations and individuals who are engaged in the struggle to better the conditions under which we work. I believe that that can be done. We know that there are legislative questions now before the Board of Trade and other State Departments which are menacing the development of the electric traction industry, of these power-distribution schemes, and of electric lighting. I hope that the reception which members will give to this paper will strengthen the hands of the Council by enabling it to feel that the whole body of members is behind it in lending the weight and the authority of this Institution to the support of remedial measures calculated to remove the disabilities under which our industry is labouring.

Mr. J. S. Raworth: The very important paper which Mr. Madge

Mr. Raworth. has brought before us covers such an enormous number of side issues that I shall confine myself entirely to a consideration of two questions. Are we behind the position we ought to have achieved? and, What are the causes that have brought about this lagging? In a meeting like this, which generally devotes itself to scientific questions, there must be many present who, as it were, never get away from their calculations, who fail to take a broad view of the industry at large. They see that everybody around them is busy, and therefore they think that the industry is in the flourishing position in which it ought to be.

Now, let me compare our position in electric light engineering, not with the position in America and on the Continent, where the conditions may be different, but with that of the gas companies, which draw their customers from the same class of people as that which furnishes ours. Our position at the present moment is this (I am now comparing the figures of 1898, which are the only figures available for comparison): The total revenue of all electric light and electric power companies in the United Kingdom of Great Britain and Ireland amounts to £1,606,000 per annum. But it is astonishing to note that the *increase* in the revenue of the gas companies in the two years from 1896 to 1898 exceeded the sum total of all our revenue; that is to say, it increased from 19 millions to 21 millions, making a total increase in revenue of £1,611,000. I ask you to realise that in the face of all the electric lighting stations which have been started throughout England, the gas companies have been able to add one million and a half pounds to their revenue. Further, the profit which the three large gas companies in London derive from the supply of gas is £1,655,000 a year, that profit being greater than the total revenue of the electric lighting companies in Great Britain and Ireland. With those figures before them, who could say that our industry has not been choked? We have been working at it as hard as we could in supplying electric light for twenty years, and yet we have not gained more than £1,606,000. Some may say that we have been putting down stations as fast as possible. The result is, we have 198 stations working to-day, and the gas companies have 661, showing that they are still a long way ahead in numbers as well as in the breadth of their operations and their power of collecting money. Now, I have no great antipathy to the idea of municipal trading; I do not object to the municipality trading, because they bring competition into our field of operations; and I do not object to the ratepayers taking the risk upon their shoulders if they are content to do it. My objection to municipal trading is based upon the ground, which I explained at great length two years ago, that they do not manage the business as well as the companies do. I say that, in that great industry of gas, which was based upon the principle of perfect equality between the company and the municipality, there was no advantage to the municipality which the company did not also have, and the companies beat the corporations entirely, and supplied a better article at a lower price. It was only when we came to tramways and electric lighting that the new principle was introduced of giving an advantage to the corporation as against the company. And I contend

that, if those advantages on behalf of the corporations were taken away, we should no longer have cause for fear, because the municipalities would not choose to go into the field where there was free trade. It is the special provisions of those two or three Acts of Parliament that bring about the great mischief from which we suffer at present. As General Webber might have told you had he been here, between 1882 and 1888 it was impossible for any electric lighting stations to be started, because the capital could not be got. Then the period for purchase was extended to forty-two years, and so we now get the capital, but only with very great difficulty. Further, the difficulty is increased by the violent opposition of corporations to the granting of an Order on fair terms. If it be urged that there are a great number of gentlemen ready to come into the business, to put their money into it, and take the risk of electric supply, it must further be considered what a large amount of capital they have to spend in overcoming the opposition of local authorities. It was said by Mr. Vesey-Knox that if people are prepared to supply at a cheap rate they can get permission to supply over a very large district. Let them try to do it, and they will find they have to spend weeks in the rooms of the House of Commons arguing the point *ad nauseam* with people who only want to keep them out; and an enormous sum of money is spent often without any success or reward at all. Those are the obstacles put in our way by the Legislature, and if those were removed and we were put on equal terms, we should have no more trouble with municipalities. Mr. Raworth.

Mr. SWINTON: I agree with Mr. Sellon, that this is a subject of very great importance; it is a pity that we do not more often have discussions upon subjects of this description here. It is true that this is a scientific Institution, but it is no use having discussions upon, for instance, dielectric losses in cables, if we have no cables in which dielectric loss can occur. Mr. Madgen has stated in his paper the chief reasons for the backwardness of the electrical industry in this country. Up to a very short time ago—I do not think it is so now—if such reasons were adduced for the backwardness of this country, there was always a certain class of persons who denied that those were the real reasons, averring that the true reason was the speculation that took place in electric businesses about the year 1882. I do not think that the British investor has such a long memory that it takes him back to the year 1882; the usual cycle of memory of the ordinary Stock Exchange investor is very much shorter than that. There is no doubt that the primary reasons for our backwardness in electrical matters in this country are traceable to the Tramway Acts and the two Electric Lighting Acts, and, I must add, to the administration of those Acts by the Board of Trade. Mr. Madgen has stated in his paper that the Board of Trade is curiously named, and somewhat curiously constituted. I have personally been at some pains recently to inquire into the constitution of the Board of Trade, and I have had great difficulty in finding out anything about it. In any ordinary book of reference, under the heading of "Board of Trade," there are mentioned the President, the Secretary, and various sub-secretaries, but there is no reference to any Board at all. I have pursued my investigations, and find that the Board Mr. Swinton

Mr. Swinton. of Trade has been in existence for some centuries, that it has led a very chequered career, having been several times entirely suppressed, but that, as at present constituted, it consists of a Committee of the Privy Council, formed of various high officers of State, and includes, *ex officio*, the Archbishop of Canterbury! I need scarcely say that the Board of Trade appears to be but seldom called together, and in fact exists merely in name. Now, I think that that really goes to some extent to the root of the matter. At one period in the history of the country the Board really existed, and probably was of some considerable importance; for I find that Oliver Cromwell, who did much to increase the power of this country both abroad and at home, appointed on the Board of Trade twenty merchants of the City of London. If something of that kind could now be done towards constituting a Board of Trade which would be truly representative of the industries of this country, and which would guide the President and Secretaries, it would be a very good thing.

To pass to another question: it must be evident to any one who thinks about the matter that putting any new industry into the hands of bodies like municipalities must necessarily lead to that particular business proceeding very slowly. Municipalities employ the ratepayers' money. They must not speculate; they can only take up businesses the success of which is absolutely assured. At the beginning of all new enterprises there is necessarily a speculative period, and, if enterprises of that description are entrusted to municipalities, they must necessarily wait until other people have shown the way. That is what has happened in this country, and it is, I think, one of the objections that goes to the root of all municipalisation. Private enterprise is the only legitimate way of trying new things. Even if, as I do not believe is the case, the result is that, in any particular town the municipality can supply electric energy more cheaply than can private enterprise, that may be to the advantage of the particular inhabitants, but owing to the fact that it throws things back it cannot be to the advantage of the country. If, in the early days of railways, this country had been content to wait until other countries had made railways, I have no doubt that ours would have been constructed very much more economically than they were, and very likely we should now be able to travel more cheaply. But the country would have been vastly the poorer. This country was the pioneer in railways; it made railways all over the Continent, and I am quite convinced that the amount of profit that was made and the capital value of all that advantage, has been very much greater than would pay ten times over any excess in fares that perhaps we pay to-day for travelling.

Previous speakers have alluded to the fact that, at the present moment, it is not easy to raise money for electrical industries. Again, I think the reason is largely the fact that the greater portion of the electrical business in this country is in the hands of municipalities. In nearly all our large towns, except London, the electrical business (lighting, power, and tramways—or at any rate the lighting and power) —is in the hands of municipalities. Any one wishing to raise money for some electrical undertaking in the country finds that the people

will not look at London; they say it is an exceptional place, and cannot be compared with their town. But, leaving London out of account, there is no place, or at most but few places, that can be pointed to where large profits are being made, because it is all in the hands of municipalities, and the municipalities do not pretend to make profits. As showing an instance of that, I may mention Newcastle, which is a fairly large town, and in which there are two private companies, both of them very prosperous. It is noteworthy that the electric supply undertakings in two other towns, Scarborough and Cambridge, are almost entirely capitalised from Newcastle—at least two-thirds of the capital comes from there—the reason being that the people in Newcastle who had put money into electric light undertakings had found it very profitable, and were ready to invest in electric enterprises elsewhere. Hence it may be argued that, supposing the electric light of Edinburgh, Glasgow, Liverpool, Manchester, and all the large towns, instead of being in the hands of municipalities was run by well managed companies, all paying their 10 or 15 per cent., there would be no difficulty in getting any amount of money for electrical enterprise. I think that one of the things required is that some body of persons representing the electrical industry should assert themselves. I have a very high opinion of Government permanent officials, but they take the line of least resistance to a large extent, and if they are entirely pushed one way they will go that way. At the present moment the pushing is nearly all done on one side, namely, the municipal side, by the Association of Municipal Corporations, and bodies of that kind; and what is wanted is that some body should push the other way, and I think that this Institution might do this to a certain extent. Of course this Institution is primarily scientific. Very likely the Institution of Civil Engineers have never done exactly what I think this Institution ought to do; but then circumstances alter cases, and exceptional circumstances require exceptional remedies. Mr. Swinton.

Mr. LL. ATHERLEY JONES, M.P.: I must confess I came here with the idea that I should simply have to perform the agreeable duty of listening to the observations of gentlemen more competent than I am to express an opinion upon the subject-matter of Mr. Madgen's paper. But perhaps there was some solicitude that, inasmuch as an attack had been made—perhaps not an unmerited attack—upon the Institution of which I am a member, it would only be a graceful act to call upon me to say a few words in its defence. I frankly admit that Parliament has been, perhaps, remiss in the efforts which it might have successfully made to encourage and develop the great electrical enterprise, whether by way of traction or whether by way of lighting, which has had such marked success in other countries, but which in this country, through causes which have been lucidly traced by Mr. Madgen, have not met with corresponding success. But we are probably all agreed that, within the last few years, at any rate, there has been a growing conception on the part of Parliament that the interests of the community can perhaps be better served by adverting to the dictates of private enterprise, rather than the narrow and somewhat insular interests of municipalities. I am far, indeed, from saying one word which would Mr. Atherley Jones.

Mr. Atherley Jones.

reflect upon our municipal government. We owe the creation of that public spirit which finds its best demonstration in private enterprise, to the encouragement of the growth of that free spirit which exists in our municipal government. But at the same time we have perhaps coddled our municipalities a little too much. We have recognised by recent legislation that the chess-board system—if I may use the expression—of local government is not that which, in certain directions, is best suited to serve the public benefit. And therefore, instead of taking some small municipality as the unit for the purpose of local government, you have ignored that small municipality, and have merged your scheme of local government in the larger unit of the county. That has been done in various matters of local government, but it has not been done in respect of electrical enterprise, be it lighting or be it traction. Therefore it is we find that one of the greatest hindrances, probably, to the development of electrical enterprise, in traction or in lighting, has been the veto which can be exercised by local authorities over the private enterpriser. That, I think, will be removed. I think that Parliament, in passing the Power Bill in the last session for the county of Durham and for a certain portion of South Wales, recognised that the will of the municipality was not to override the necessities of the public at large. Because, if I remember aright, that Bill, or one of them, met with very considerable opposition from municipal authorities; and I understand that similar Bills in this coming session of Parliament are likely to meet with similar opposition.

There is one other topic of Mr. Madgen's paper which struck me very forcibly, and that was where he advocated the desirability of developing and encouraging electrical enterprise in the interests of the huge masses of our population aggregated in labour districts. I believe, and I think that that is the opinion even of that much-abused body, the Board of Trade—whatever the Board of Trade may be—that it is desirable that facilities should be afforded, to what length perhaps it is difficult at present to say, for tramway companies and light railway companies to construct, under more favourable conditions than at present, means of transit from the centres of industry to those more distant places in which alone cheap habitation can be obtained. And if the present President of the Board of Trade devotes his attention in this session of Parliament to making successful the Light Railway Act, which is to some extent, I believe, to supersede the present Light Railway Act, and which might perhaps reasonably supersede in some respects the Tramway Act—that will lead in no small measure to the development of electrical enterprise in this country.

I will only say it has been a great pleasure to me to listen to the interesting observations which have been made. I suppose I may be permitted to say I have been able to induce the Board of Trade to receive a deputation to discuss the precise scope and direction of the new Tramway Act, and that these matters which have been dealt with so ably by Mr. Madgen will undoubtedly be laid before the Board, and I hope will bring forth good fruit.

Professor Silvanus Thompson.

Professor SILVANUS THOMPSON: We are all indebted to Mr. Madgen for having brought before us this topic, for I doubt if a more important

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subject could have been brought before this Institution at the present time. It is really a national question—this matter of being able to supply power cheaply to our industries. And I would like to point out that there are one or two small, comparatively simple, matters, in which it is desirable that we should have our minds quite clear. It is sometimes urged against us when we advocate the establishment of these large power-stations, and when we adduce, as practical reasons why they should be encouraged, those large power-stations on the continent of Europe and in North America, that those stations are almost all without exception water-power stations. They say, "You have no waterfalls in England worth having, and you do not know anything whatever about working power-stations except those which have water as the natural source of supply." I do not admit the argument. In the first place, if there is a waterfall that is not yet taken for power purposes, that waterfall belongs to somebody, and that somebody will want his price for it so soon as it is known that the power of the waterfall is worth money. Whoever wants to take that waterfall for water-power purposes will have to pay for it; and the price which he will have to pay is that which he would have to pay to run in some other way the works which are going to be run. That is the factor which sets the price. You do not get water-power for nothing, although Nature provides you with the water falling over the precipice. We have in this country, as everybody knows, cheap coal; coal which Nature also provides free of cost, but which belongs to somebody, and which costs us money for digging up. The economic problem is not coal *v.* water-power, but how to make use of the natural source of power which we have, whether it be coal or whether it be water. The engineering may be different, but the economic problem is really precisely the same. And the dearer the power is, as Mr. Madgen has well remarked, in itself, the more there is to be gained by distributing that power as economically as possible. So that if it be true that power costs us more by being generated from coal than by being generated from water flowing over a precipice, it is all the more necessary then that we should have an economical system of transmitting it and distributing it.

Another practical point is, that we want information, and I will ask Mr. Madgen in his reply to give it. Where are there (I know of some) large stations worked by coal as distinct from large stations worked by water? I do not ask, Where are coal-stations for the purpose of sending power to a long distance? That is not the question. But, Where are there large stations economically transmitting electric current to the district round them for any purpose whatever? We want to know the figures for large coal-stations in comparison with those of the water-power stations when we have to talk to the people who are impervious to the argument that water-power cannot be got for nothing. Then another thing we want to know is, What is the proper basis for reckoning out the suitable size of unit for a big power-station? Of course I know that no one answer can be given: it depends on local conditions. But when we are told that a municipality intends to supply power as well as light, and that then because it has a station big enough for 1,000 H.P. it will therefore

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be able to compete economically with one of those large power-schemes which has a big central-station a few miles away, we want to know, and have the facts for it, Where is the size of station at which it becomes no longer economical to make it larger, where it would pay better to put a second one up at some other convenient place on another coal-mine a few miles away? Unquestionably facts can be got at, but they have never been stated, so far as I know, in a compact, concise, or useful manner, that could be used effectively for the purpose of convincing those who do not understand what the real problem is. Is it true, for example, that when you get a station up to the size, as I have been told—I do not accept the figure—of 10,000 H.P. it does not pay to go on to make it up to 20,000 H.P., and that you had better put up a second station a little way off? You know that directly you double the station by dividing it into two at a distance apart, certain expenses are doubled, which would not be doubled if the station remained as one large station. I need not go into the details of it, but it is obvious that there must be additional expenses whenever you break ground in a new place. Is there, or is there not, a limit of size beyond which the increased output will no longer produce a cheaper output? Our municipalities up and down the country sometimes use rather curious arguments. One that I have heard used, rather effectively, against a big power-station coming into that municipality was that, seeing that they were makers of their own gas, they had a very large quantity of coke, which they sold every year, and on the sale of their coke at a cheap price they made profits, and that no power-station twenty miles away could possibly dig up fuel for itself which would be cheaper than the spare coke which they themselves produced. Of course, the obvious answer to that is that that municipality is making huge profits out of the gas that it sells; it is not selling that gas nearly as cheap as it might do, but it is taxing the community for the gas over which it holds a monopoly, and throwing away, or selling very cheaply, a waste product, that need not be in that sense a waste product. The economic unit of size for electric supply must clearly depend upon very different conditions from those which determine the economic unit of size of area supply for gas, or water, or milk, or other things. Take, for example, the case of water supply. In old days we had the parish pump, and then we had the village cistern, the village reservoir; then we had the town supply. Now look at what all the large municipalities in this country are doing in the way of providing themselves with immense water supplies from outside their own area. The same kind of progress, as social conditions change, is produced in other things beside water-supply. Last autumn, at the time of the British Association meeting at Bradford, I was inquiring into some of the conditions which affected, or would affect, the supplying of power in Bradford, and one that is not to be despised in considering the question of area is this: In the town of Bradford practically the whole of the industries that require power are textile industries, and all the mills practically begin work and leave off work at the same time. Any one who knows anything about electrical supply companies knows that that is not the most favourable condition

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for the supply of electricity; for you get a load-factor which is undesirable, as in the case of lighting, when all the light comes on or goes off at the same time. It is really much better to have a consuming element which takes power from the station for a variety of industries which do not all start off at the same time, or stop at the same time. Therefore, in determining the area which would be economical for the supply of electric power, it is inadvisable to confine that area to a place where the industries are all of one kind. It would obviously be more economical to include other towns with the town that had one industry—other towns which had other industries which do not want the supply at precisely the same time, because every one knows that what will fill up the gaps in the electric light load between the demand of one consumer and that of another, is all to the good of that electric lighting station, and improves its load-factor. When we compare the state of things in Italy, for example—a relatively poor country, a country very far behind in many ways—and see how, in Italy, station after station has been built, each station with very large generating machinery distributing power over wide areas, one feels perfectly ashamed of what is going on in our own country. I found in Bradford a firm which supplies power-looms to the textile industries of Lombardy, and had supplied some thousands of looms quite recently, within the last year or two, every loom being fitted up with an electric motor, I believe in ninety-nine cases out of the hundred with a three-phase motor on the end of that loom. They were supplied from Bradford, the very centre of our English textile industry, yet the same firm has not supplied a single loom fitted up with an electric motor for this country! It is most astonishing that we should be sending to Italy, to compete with us, looms electrically fitted, because they have an electric supply, and that in Bradford there is not a single loom fitted in that way. I leave you to consider why.

We have been told that we are a nation of amateurs. The reader of this paper has emphasised that point. But I do not quite agree with him in thinking that it is a matter to be lightly thought of, that we do not put the same store on education as some other nations. If you will go to one of the large factories that send over machinery to this country from the United States you will discover that practically every man in that factory above the grade of fitter is a college graduate. They do not turn a man away there because he is a college graduate, or sneer at him as being unpractical. No, they welcome him, and take him in, and make the best of him, with the result we all know. In Germany and in Switzerland one knows that throughout the factory, in every department, you will find as managers of the different branches men who have received the highest scientific and technical training. It is brains really against which we have to fight, alike in the case of the German, the Swiss, and American competition in these matters. They do not leave things to be "muddled out." They do not leave them to be done in an amateurish way. They think it out beforehand, and they take the best-trained brains and make the most use of them.

One argument that I have found very effective in talking to the people who think that a little electric light station can supply power in each

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little town up and down the country without any big power-scheme, is this: See what has happened in London. In London we began with our Vestries and Companies putting down little stations here and there, one in each of our parishes. Gradually these things are coming right, and are being altered. We are finding that a central station is an absurdity, that no station ought to be central, that all ought to be out of the centre, that they ought to be outside; that we ought not to have to be carting coals into the middle of a great population, and carting ashes out again; that the economical place to put the station is outside, and that a few large stations outside are paying better than a great many little stations each in its own district. When you point out that, then the doubter begins to see that a large supply station serving a large area is really a much more economical thing than a number of little stations dotted down in the different towns.

May I conclude with a parable? Walk down Aldgate, and you will see the pump, the famous Aldgate pump, surviving to this day, and reminding us of the time when each little bit of a parish had its own water-supply. The time is not far distant when any Vestry Electric Station, or any petty little town station, will be looked upon as just as absurd for the purpose of a big power-supply to the industries that want mechanical power cheap, as the Aldgate pump is for supplying water to London.

The President.

The PRESIDENT announced that the scrutineers reported the following candidates to have been duly elected:—

Member:

Montague Brown Mountain.

Associate Members:

Henry William Clothier.
Matthew Clavering Coates.
Henry William Wartnaby Dix.
Alfred Ernest Kennard.
David Henry Kennedy.
Frederick Joseph Arundel Matthews.
John McLellan.
John Frederick Nielson.
Arthur Peckett.
Herbert William Sprunt.
Noel Statham.
John Erskine Meysey Stewart.
John William Towle.
Charles Tuson.
Henry Dewar Wight.

Associates:

Harold John Bullock.
Frederick Thomas Callis.
Charles James Carter.
Charles Henry de Russett.
Arthur William Fithian.
Thomas Gemmel.
Thomas John Grainger.
John Angus Hay.
Colin Bell Heaviside.
Mukand Lal.
Donald Macdougal Macbean.
Colin Campbell Macmillan.
Stephen George Martin.
William Odgers.
John Edmund Pownall.
William Pummell.
Bernard Rance.
Frederick Rawlings.
Maurice Hugh Reynolds.
Lionel Inglis Robinson.
Edward Alexander Savage.
Walter Simpson.

Henry Beven Swift.
Thomas Fane Tebbutt.
H. Arthur Thomson.
Alexander Houston Weddell.
Richard Ffolliott Willis (Capt., R.M.L.I.).
Herbert William Wilson.

Students:

Herbert Addy.
Ellis Amos.
Herbert Dudley Ash.
Claude Randolph Barry.
Frederick Edmund Berry.
Edward George Paul Bousfield.
Swinfern Bramley-Moore.
Jeoffrey Thorold Brookes.
Harold Thomas Brown.
Samuel Wilfred Carty.
Edwin Olding Chadwick.
Albert Bernard Clark.
Benjamin Charles Colley.
William Michael Conway.
Denis Cullen.
John Stewart Dow.
Hugh Victor Diamond.
Percy Farmer Draycott.
Alfred George Ellis.
Christmas Llewellyn Evans.
Thomas R. R. Gaunt.
Edward Lind Gosset.
Charles Ernest Greenslade.
Herbert Ernest Hart.
Thomas Hopper.
Hugh Arnold Hughes.
Walter Ings.
Albert Henry Jackson.
Henry William Jones.
Frederick Edward Kennard.
William John Kinnersley.
Raymond Vincent Marriner.
Arthur John Martin.
John Edward Medley.
Percy George Mitchell.
Ernest Josias Nichols.
Patrick O'Hara.
Henry Arthur Pickett.
Amos William Pulvertaft.
Frederick Charles Purvis.
William Bradley Randell.
Thomas Raven.
Ronald Morrice Robertson.
Lionel George Frank Routledge.
Herbert Samuel Selves.
Arthur Greystone Shearer.
Ernest William Short.
Percy Rayner-Smith.
Samuel William Steane.
Walter Alexander Turnbull.
Stanley Thomas Walker.
Arthur Percy Whitehead.
George Wyatt.
Arthur Primrose Young.

The Three Hundred and Fifty-Ninth Ordinary General Meeting of the Institution was held at the Institution of Civil Engineers, Great George Street, Westminster, on Thursday evening, February 28, 1901. —Professor JOHN PERRY, F.R.S., President, in the Chair.

The minutes of the Ordinary General Meeting held on February 21st, 1901, were read and confirmed.

The names of new candidates for election into the Institution were announced, and it was ordered that the list should be suspended.

The following transfers were announced as having been approved by the Council :—

From the class of Associates to that of Members—

Michael Birt Field.

From the class of Associates to that of Associate Members—

Albert W. Makovski.
G. C. F. Székács.

Donations to the *Building Fund* were announced as having been received since the last meeting from A. D. Williamson, H. B. Graham, C. Poulsen, A. H. Unwin, J. W. Manley, and Captain R. Willis; and to the *Benevolent Fund* from C. J. Carter, R. J. Wallis Jones, and Captain R. Willis, to whom the thanks of the meeting were duly accorded.

Mr. H. W. K. Irvine and Mr. R. P. Brousson were appointed Scrutineers of the ballot for the election of new members.

Professor W. E. AYRTON: I rise to propose no ordinary vote of condolence. For in expressing in conventional form the sincere regret which this Institution feels in the death of the Chairman of its Dublin Local Section, I desire rather to give voice to our deep grief and sorrow at the loss of the dear friend of many, and the willing helper of us all.

To some of you, perhaps, Fitzgerald was but a name, and why? Because his life's work was that of inspiration—inspiring others. He gave them his ideas, aye, he even explained to them the real meaning of their own. Science to Fitzgerald was what religion is to the highest type of priest—not a thing to be used to enhance the status or the wealth of its exponent, but as a great good to be poured forth without stint and without reference to reward or even recognition, for the benefit of man and of the world. Singularly free of all human ambition, singularly fertile in suggestion on all subjects, was our dead comrade. He knew his worth, but never claimed it. He knew his power, yet, whene'er he used it, it was for you, for me, for science, to secure intellectual liberty, but never to secure position for Fitzgerald. And yet for him abstruse mathematics had no difficulty, complex physical processes no obscurity. He seemed to be able to follow the interaction of invisible molecules more easily than we can grasp the working of visible machines. This morning I was looking at two letters from him, one which reached me the very day that Mr. Duddell read his paper in this room in December, and the other a few days later. Every line breathes suggestion, encouragement, criticism, modification. Why, those two letters are a little treatise on the theory and the possibility of Mr. Duddell's devices. Single-minded, simple-hearted, he died as he lived, respected, honoured, beloved.

General C. E. WEBBER: I cannot pretend to follow such an eloquent discourse as you have just heard from Professor Ayrton, but I would say that there is a feature in connection with our dear late friend, Professor Fitzgerald, as regards what produced that man, which is of some interest. Although heredity may not be strictly scientific, it is a subject which attracts us all. Professor Fitzgerald could trace his ancestry back to the Normans who, starting from Scandinavia, conquered Normandy, conquered Italy, conquered England, and finally conquered Ireland. There can be little doubt that the brain work which we have just heard described, emanated from tissue that was strengthened and was brought down to him through generations of strong and powerful men. But not only was he strong. He was essentially a man who

was entitled to be called gentle. Gentlemen, I beg to second the motion which will be read to you by Professor Ayrton.

Professor SILVANUS THOMPSON: I wish to add my voice to that of Professor Ayrton and General Webber in expressing condolence with the loss of one who was so dear to us personally, and so very helpful to every worker in science who had the good fortune to know him, however slightly. Not very long ago I was talking with some of the scientific men of Germany, and they were deploring that they did not, or could not, do in Germany that which is done so very effectively in England by the combined efforts of scientific men working together in Committees. They instanced the Committees of our British Association as examples of the voluntary associations which did from time to time excellent work, carrying that work on in continuity from year to year in a way which did not happen in Germany, where the only combined kind of effort was that which was done under the patronage either of the Government or of one of the Universities, and mostly by paid workers. Among the members of the British Association, whose help on Committees of that kind when organising scientific work to be carried out during the course of a year, if there was one more helpful than another it was Fitzgerald. We ought not to forget what a very important part Fitzgerald's ideas played in the early development of the question of the production of electric waves. Before the researches of Hertz, which resulted in the experimental methods of exploring electric waves, Fitzgerald had read at least two papers before Sections of the British Association, suggesting ways in which such waves might actually be started; in fact, suggesting beforehand that which Hertz accomplished, and predicting for us the generation of these waves on the lines of Maxwell's investigation. I believe I am right in saying also that one of the suggestions for a coherer for detecting waves, if not actually the first, was made by Fitzgerald himself. In thousands of ways did he help others to carry out ideas, and gave them ideas to carry out; ideas which he could very well have carried out himself. But his mind was so fertile that he could not ·ly, even had he had the time at his disposal, have

carried out one-twentieth part of the brilliant suggestions that he gave freely to others and to the scientific world. He was a man who was to a certain extent overloaded with administrative and educational work that ought never to have been put upon him. He ought to have had freedom for research, and ample means for carrying out researches. Whatever he did he touched with the finger of genius. There are few of us who may claim in any sense of the word to have been workers in physics in the last fifteen years, who do not feel a debt to Professor Fitzgerald. I had the good fortune to be associated with him very closely during the last three years in quite a different branch, namely, as co-examiner with him in the University of London. And even in the routine work of examining candidates for their degree in physics, Fitzgerald was as fertile in suggestion, as kindly, as helpful, as any man could possibly have been. A truer or better colleague in that capacity one could not have desired. I remember very well indeed being struck with the pains with which he read through, and referred in detail to, and made careful investigations himself about, the theses which were presented for the Doctorate of Science by candidates in physics. It was characteristic of the man that when sitting as critic upon a work presented as a thesis for examination, he should act as helpful critic, and as a man of useful suggestion. There are few men in this world like Fitzgerald : there are few indeed who can claim the ability in any walk of life which Fitzgerald exercised in the highest branches of physics : there are few who equalled him, none whom I ever met who surpassed him, in kindly helpfulness to others.

The PRESIDENT: At the inaugural meeting of the Birmingham Local Section of this Institution last night, I said what I have to say to the Institution on this subject. It will be printed, I hope, in the Journal. I should have gone to Dublin on Tuesday for personal reasons, but I went to Dublin as representing the Institution at the funeral. It only remains for me to read Professor's Ayrton's motion to the meeting :—

"That the Institution of Electrical Engineers, in full Meeting, desires to express its profound sorrow at the death of Professor George Francis Fitzgerald, and to place on

record its high appreciation of his brilliant qualities as a man, as an investigator, and as a leader of scientific thought and to express to Mrs. Fitzgerald and his family their heartfelt sympathy under the calamity which has fallen on them and on Science."

The resolution was carried in silence, all present standing.

The President. The PRESIDENT: I will now call upon Mr. Hammond to resume the discussion on Mr. Madgen's paper.

Mr. Hammond. Mr. R. HAMMOND: During the discussion on the Power Bills last session, before Sir James Kitson's Committee, some vitally interesting engineering questions arose in connection with the generation of electrical energy at centres, and the distribution of that energy over large areas. Those questions still await discussion and decision. I have made a note of one or two of them. What is the limit of distance from the generating works of supply to consumers, beyond which the extra costs of distribution counter-balance the advantages of concentration of plant? What is the limit of kilowatts installed in a central station, beyond which economy of centralisation of plant ceases? That question arose on more than one of the Bills, and there was a very great difference of opinion with regard to it. There were those who urged that beyond 5,000 kws. there was little advantage in concentration; there were those who urged that beyond 10,000 kws. there was little advantage; and there were those who urged that you might go on almost indefinitely, and that every time you put down plant for another thousand kilowatts you gained an advantage in cost of production. I say that that is a question which would form a very interesting one to discuss in this room. Thirdly: What are the means that are going to be adopted in order to preserve over a large area, with various consumers, large and small, pulling at the line, uniformity of pressure so as to be able to give electric light within, I will not say Board of Trade limits, because this paper does not like the Board of Trade, but I will say within efficient limits? I am sure that we all feel that when we take a residence in the North of London, and we are supplied from the Northern Metropolitan Power Company, we shall not like our light to be pumping up and down, and we shall certainly have a right to consider before we turn it on what are the means that are going to be adopted in order to give us just that little variation of pressure which we, as electrical engineers, can put up with. I mention those three points, each one of which I think could very seriously occupy the attention of this Institution. They are certainly within its direct scope. and I feel confident that when the engineering questions arising out of these Power Bills are discussed, our guests from the country will find this room full to overflowing. To-night we are dealing with a different subject. This paper has an excellent title, "The Electrical Power Bills of 1900: Before and After," but not one of the engineering questions which we, as electrical engineers, are interested in, is touched in this paper. That leaves a chance for somebody else who is interested in the

engineering side of the question to come up with another paper—that will be "The Electrical Power Bills of 1900: After," leaving out the "Before." This paper, I think, may be summed up under about four headings, and these four points are, I conceive: (1) in the first few pages, a lament, a dirge I may say, over the fact that we in the United Kingdom are terribly behind in the matter of electric lighting, fortified by certain extracts written by the most irresponsible persons in most irresponsible daily papers; (2) an intimation that this backwardness in electric lighting is due to the ignorant Legislature, the foolish Board of Trade, and the selfish local authorities; (3) an intimation that what we should do is to copy the German and American methods; and (4) an appeal to this Institution, fortified by chapter and verse out of its Articles and Memorandum of Association, to form a committee in order to make the path of the Company promoter at all events a little smoother. As far as I am concerned, I am very sorry I was not at the reading of the paper, because I should have liked to get in at a very early part of it. I was professionally engaged elsewhere, but I understand that almost all the speeches were thoroughly in support of the paper. Therefore, I am glad to be here to-night, to say that I distinctly dissent from those four propositions, though, of course, there may be more than four in the paper that I dissent from. I will take the first. Why are we behind in this country in the matter of electric lighting? The paper gives certain explanations. I deny the soundness of those explanations. Why are we behind? Let any electrical engineer in this room who has had to face a Board of Directors desiring to put their money into electric lighting (or unwilling, as the case may be, but anxious at all events to make a good investment), say why; or let him face a committee of a Town Council equally desirous of extending the town investments, but not wishing to make a mistake, and then give his opinion on the subject. What is the feature that has kept us so far backward. It seems to me to be the A B C of the subject. Those of us who have passed through it have always been met with one point, and that is, we cannot compete with gas at 2s. a thousand feet. That was said to me by a number of capitalists in Leeds ten years ago, and the result was that they delayed starting works. When I went to the Corporation, and tried to persuade them, they equally dissented, because gas at 2s. a thousand feet could not be competed with, and over and over again in this country up to the present time the spread of electric light is checked by the keen competition with the cheapest gas in the world. People have come back from their summer holidays and told me over the dinner-table that they have been at a small village in Switzerland, and they have seen the electricity in the rooms of their hotel. But how is it? It is because they cannot get gas at any price. When I first went to New York I found gas costing 10s. a thousand feet, and I was not surprised that Mr. Edison was doing so well with his electric light. Throughout America and on the Continent you find high prices of gas. I took my Dublin friends across last year to the Continent, and there was one man who would ask, "What is the price of gas?" And when he found that it was 6s. or 6s. 6d. he was amazed. The real factor in the backwardness of electric light in this

Mr. Hammond

Mr. Hammond. country is not the factor mentioned by Mr. Madgen, but the cheap price of gas. Mr. Madgen says that it is all due, or almost all due, to local authorities. I ask you to say that that is an absolutely mistaken idea to nourish. Local authorities in this country, out of investments amounting to twenty-five million pounds, have put down fourteen million pounds. We owe as an Institution a great debt of gratitude to the local authorities in this country, for the customer who comes and puts fourteen out of twenty-five millions into an industry is a desirable customer and we, as electrical engineers, owe him a debt of gratitude. As for the consumers, the local authorities have shown the companies how electricity can be produced at a low rate. Their average rates of charge are far lower than those of companies. There are two undertakings in the country that supply at below 3d., and both are local authorities. There are 6 that supply between 3d. and 3½d. None of them are companies. There are 12 that supply between 3½d. and 4d.—nine local authorities and three companies. There are 30 that supply between 4d. below 4½d.—25 local authorities and 5 companies. There are 23 supplying at 5d.—18 local authorities and 5 companies. If I had time I would carry the point further, and show, when you get above 5d., how largely the companies exceed the price of the local authorities. Now for Germany. I say with regard to Germany, let us do as the Germans do. My work has taken me three times to Germany this year, and I have gone from one central station to the other, and I will tell you what you are likely to see when you get there. With regard to the wonderful power schemes in working order mentioned by the author, you will not see one. You cross the frontier at Aix-la-Chapelle, and you will find that the works are owned by the Municipality. You stop the first night at Hanover. You go on to Dresden, and you put in a few hours at Nuremburg. You stay at Frankfort, and then you go to Mayence—and in every case you find that the works are owned by the Municipality, Finally you stop for a night at Mannheim, and discover that there is there a big power scheme supplied by the Municipality. Finally, I disagree with Mr. Madgen, who says that we want a committee to put these local authorities right, and to straighten out the Legislature. No, better far for some member—I will not, as I am on the Council—to move an amendment that a committee be formed to consider engineering questions connected with power supply, and to report to the Institution to what extent the present power schemes are likely to overcome them.

Mr. Morse. Mr. S. MORSE: I hoped to have spoken later in the evening, but it is perhaps as well that some one should at once get up to suggest to the last speaker that his speech was one which it would have been better to have left undelivered. I regret that it was put forward by the last speaker that he would not, by reason of the fact that he was a member of the Council, move an amendment. I should have said he did not dare move an amendment. I know no reason whatever—and I have been a member of the Council myself—to prevent a member of the Council moving an amendment to a resolution moved by members of the Institution. I should have thought that if a member of the Council felt a strong opinion upon any matter, it was his duty, as well as his privilege, to move the amendment.

I have had an opportunity for some years of knowing something about what has been going on in these matters, and I say without hesitation, or without fear of contradiction—because I do not attach the slightest weight to what has fallen from Mr. Hammond on the point—that the present position of the electrical industry of the country is largely due to the unreasonable difficulties put in the way of its progress by local authorities. I should not have discussed the matter from this point of view if it had not been for the remarks made by the last speaker. He has chosen to put it forward: and no one can deny that our backwardness is almost entirely due to those difficulties. I agree that Parliament also is partly responsible, but it must be known to members here to-night that the local authorities have gone further than acting merely within their own districts as regards what ought to be the welfare of those districts, and have entered into such an alliance as ought not to be permitted, and have put great pressure upon Members of Parliament, when matters of this sort have come before Parliament for discussion. I am not an electrical engineer, but I do know this, that it is essential for an industry of this kind that there should be available to every manufacturer throughout the country a supply of cheap power, available to him at all times of the day and night, at rates which will enable him to compete with his foreign competitor. Why is not that here to-day? It is not because gentlemen have not the means at their disposal, and have not taken steps to obtain those powers. Every effort has been made, and what has been the bugbear to stop it? The local authorities. Look at what happened at the second reading of the Power Bills last session. What is the desire of engineers here? It is, I submit, that electrical engineering should have fair play, free play, and that schemes should be discussed on their merits. If the local authorities will agree to that proposition, instead of private companies only having a total capital of 11 millions out of 25 millions invested in such enterprises, we shall have a total of more like 250 millions. I am astonished at any one in the position which Mr. Hammond holds saying that 14 millions spent by local authorities in this country is a large sum. It is an extremely small and insignificant sum, and one which makes the traders of this country ashamed of the position which they hold in the world. I do feel very strongly that we are here dealing with a question of the utmost importance. I do trust the Institution will put forward, as the word which should go forth to all parties in the matter, their opinion that the trade should have free play. Let the traders have a right to have their schemes discussed upon merits, and abolish the veto which unreasonably and improperly has been given to local authorities to prevent that being done.

Mr. G. L. ADDENBROOKE: It is rather difficult to speak at this stage of the discussion. After having had, I think, almost as long an experience as any one of work on these large power schemes, I may say I am very largely in agreement with Mr. Hammond, at any rate as regards his facts. The only thing is, I think he has drawn the wrong inference from them. It must also be pointed out that Mr. Hammond was discussing electric lighting altogether as if electric power had only

Mr. Addenbrooke.

Mr. Addenbrooke.

just come in. But electric power has been applied on the Continent and in America for seven or eight years now, whereas we have practically done nothing in this country. We are not discussing electric lighting at all. I quite agree with Mr. Hammond that English electric lighting has had enormous obstacles to overcome; but whether the municipalities have overcome it better than the companies would have done is another matter. I agree with Mr. Morse that if private enterprise had been conducted under proper regulations, which might have been drawn up, it would have advanced a great deal further than it has done. Mr. Pember told me some three years ago, that he was confident proper regulations, which would put private enterprise and municipal interests on a fair basis could be arranged. If such an Act had been passed in the early days instead of the Electric Lighting Act, we should have had far more than £25,000,000 invested in enterprise connected with electrical industries. The way in which electrical enterprise has been trammelled has been shown by these Power Bills. Notwithstanding, as Mr. Hammond says, that they may be very speculative enterprises—as undoubtedly they are from many points of view—and there are enormous difficulties to be faced in the way of dealing with local authorities—yet the moment there was an opening, English capital has been ready to embark on the venture. I have made a rough calculation that by the end of this session, more than £100,000 will have been spent merely in the promotion of those Bills. It is perfectly true, as Mr. Hammond says, that you may go to Germany and Switzerland and find things existing as he told you. I have made many careful inquiries as to their power schemes. When I was engaged on the Midland Power Scheme, I was asked to go abroad to report on the Continental work which would bear on it. I first of all met Mr. Kapp in Germany and talked the matter over with him, and afterwards, by his advice, went to certain places in Germany and Switzerland. I showed the various people the plans of what we proposed to do; I showed them to Messrs. Brown Boveri, and to the Oerlikon people, and said I wanted to carry out a project of this sort, without overhead wires, but with cables, and asked what they had to show me that was most like this. I may say that there was a great deal of difficulty in showing me exactly the same thing. It has always been my feeling from the very first that conditions in England are different from those on the Continent, and in America—that in these Power Bills we were meeting a set of local conditions, and a problem of our own, and that if we met that problem as I feel confident we are now meeting it, we should be in the front rank again of enterprise and in the benefit which we should confer on the country. Mr. Hammond, therefore, is right that this is a difficulty. You may see hundreds of waterfalls here and there with a local line of transmission; but only at Rheinfelden and one or two other places did I see that kind of scheme which we are trying to work out of having large power stations in the centre, and a large industrial population round about it. That is an English proposition. It naturally follows, that if you can supply power to one portion of the circle, you can supply it all round. I should have thought the proposition was self-evident. Mr. Hammond was right in saying that we have an

individual problem. We ought to be proud that we have something to do with it. It would be a grand thing if local authorities and Parliament would help those who are trying to place England again in a good position in the manufacturing world. With regard to the questions which Mr. Hammond mentioned as suitable for discussion by the Institution, they are of course most important questions. I have spent years upon them, and my feeling about these power schemes, is that every one must be considered on its own merits. They are great undertakings, most complicated, with varying conditions, depending on numberless circumstances which you cannot treat generally. All you can do is to secure a body of men who have, or who ought to have, a knowledge of their profession, and who, in consultation say this ought to succeed, and who then, like Englishmen in the old days, put down their work and their money and tried to make the thing go. That is the way in which this country has been built up, and unless the local authorities can see absolute detriment to themselves, or that the work that is to be done by the public companies is an absolute disadvantage to them, a free hand as far as possible should be allowed. We do not want monopoly. We are perfectly prepared for the local authorities to set up their own electric-lighting stations and run their own mains into all our customers' houses if they like. I do not agree with everything that is done by those promoting power-schemes; but those who have gone into the question feel confident that the principle on which they are working is to a large extent right. Even if it is wrong, if people are willing to stake their money on it I do not think it is the business of anybody else to say that they shall not, if by doing so they confer, as undoubtedly they would, an enormous benefit on the district. If they choose to throw their money away on the district, that is their concern. Mr. Addenbrooke.

I have travelled a great deal in the Colonies and about different parts of the world, and I am certain of the wonderful development which will take place due to this power work. I could tell you stories, if there were time, of the difficulties of transport and other matters which can be got over by electric transmission. We are not permitted to have any opportunities of true electric transmission in this country. We are so hampered by conditions that any scheme carried out in this country would not be anything like electrical transmission of power in other parts of the world. People come from different parts of the world, and say they want so and so, and ask what we can do? We have nothing that we can show them here, but are obliged to refer to some American or German account of a scheme that has been carried out abroad. That is the position I have been placed in in working for Australia and Africa; and I myself have had to go to America and Germany for the materials with which to carry out the work.

Mr. E. GARCKE: The great difficulty in discussing questions of this complexity always is to find people who have sufficient versatility of experience to take a fair-minded view. I was exceedingly disappointed, therefore, to hear the speech of Mr. Hammond, because if any gentleman in this room has had a varied experience of this industry it is Mr. Hammond. I had hoped that the very large and varied experience Mr. Garcke.

Mr. Garcke. which Mr. Hammond had gained in so many different branches of industry would have enabled him to give us the benefit of a fair-minded view of this very difficult and complex question. What did he do? He started off by pointing out that Mr. Madgen had offered four particular reasons why this industry was more backward in this country than in foreign countries, and he promised to give us an answer to each. It is true he only had ten minutes, but he confined himself to answering only the first of his propositions, and in attempting to answer that he offered explanations which I hold to be misleading to the younger members of this Institution. They were not so to me, nor to Mr. Morse, nor, I am sure, were they so to his fellow-members on the Council, but to a large number of students, the rising generation of this important industry, I think that Mr. Hammond might have rendered this industry and this Institution a higher and a better service. What did he say? He wanted us to understand that the Corporations had been customers of the electrical industry to the extent of fourteen millions, and implied that, if the Corporations had not expended that capital, that capital would never have been expended. Why should we owe any debt of gratitude to local authorities for that capital when, as a matter of fact, we, as capitalists, were prepared to spend that capital, and would have spent it five or six years earlier? It is not simply the amount of capital that they have expended, but it is the tardy and vacillating manner in which they have proceeded with the work. I do not wish to enter upon the question of municipalisation, for and against, it is not germane to the discussion; but there is one very important aspect which ought to be mentioned whenever the question of municipalisation is touched upon. And having said the few words on the subject that I have said, I must not sit down without saying in the most forcible language that I can use, that I am strongly in favour of municipal enterprise, and always have been. I advocate the higher development of the municipal function. I think it is the duty of ratepayers in their collective capacity to do whatever they can for the promotion of the health, the happiness, and the material comforts of their districts, but I say it is not one of the functions of municipalities or of local government to effect a transfer of the profits of trade from private companies to the collective capacity of local authorities. The function of the Government is an entirely different one. If once we ignore this, I may say, elementary economic principle of expecting the whole of our trading profits to be made by municipalities, and allow the local authorities to become the common traders of this country, there will very soon be a limit to all enterprise. I repeat again, that I express that view in opposition to municipal trading, notwithstanding that I am a most ardent and consistent advocate of municipal enterprise. I hope I have made the distinction between those two phrases perfectly clear.

What did Mr. Hammond say about the real reason why electric lighting had not developed more quickly in this country? The price of gas!—The price of gas was so low that electricity could not compete with it. Are not the millions of electric lamps that have already been installed competing with cheap gas?

Mr. HAMMOND: I meant as compared with the price; I quite approve of the competition. Mr. Hammond.

Mr. GARCKE: If I had the time I would undertake to refute the statement that gas has been any obstacle to the introduction of electric light. I remember Sir William Preece saying that the electric light was capable of being made the poor man's light, and I have preached that doctrine throughout the country, and I have heard Mr. Hammond do the same, and yet we are told that the low price of gas is a preventative to the development of the electric light. I say it is a most misleading statement. Then another argument that Mr. Hammond used was, that Corporations had supplied electric light at a lower rate than Companies. But the whole of my argument is, that if Corporations undertake to supply electric light at all they should do it in the common interests of the community, and do so, if the ratepayers wish it, for nothing. Mr. Garcke.

That is the ideal development of municipal enterprise, and therefore, the lower they go down in the price of electricity the better they are doing their work. It is on those grounds that I say municipalities are able to carry out many of these public services better than companies. But there is a period in every industry, and we have gone through it in the electrical industry, when local authorities are not able to take up the speculative risks of entering upon a new industry, and during that period it has to be undertaken by private enterprise. Private enterprise should, while it undertakes these risks, be encouraged, and not, as has been the case in this country, discouraged by the local authorities. Then Mr. Hammond referred to the Continent. But he did not tell you how the local authorities behave in that country towards private enterprise. There is absolutely no discouragement of private enterprise, notwithstanding that there is municipal enterprise.

The paper which we are discussing is an exceedingly interesting one, and the questions which it contains as to why England is backward in electrical industry—that it is backward I do not think there can be any possible doubt—but the reasons why it is, are due to a plurality of causes, and it is very difficult indeed in the course of a few minutes to refer to all the social, political, and enconomical reasons which go to make up the causes for the backwardness of the country.

Professor W. E. AYRTON: On the last occasion some of the speakers prefaced their remarks by saying: "*Although* this Institution is a scientific Society," &c. I should start quite differently, and not say "although," but "*because* this Institution is a scientific Society, therefore the paper which Mr. Madgen has contributed is one well worthy of our most careful consideration." For it is a great mistake to imagine that the science of electrical engineering is alone concerned with formulæ or equations, or with Ohm's or Kirschoff's law, or at the best with electrical experiments in a laboratory. I take it that the science of electrical engineering comprises the application of correct reasoning to all that appertains to the professional welfare of the electrical engineer. But, mind, I said the application of correct reasoning. For it is just as easy to reason wrongly about a Power Bill, as about the power used in dielectric hysteresis. It appeared, I venture to think, that there was Professor Ayrton.

Professor Ayrton.

a certain obscurity of logic characterising the eloquent speeches that we heard last Thursday, when it was endeavoured to be proved that the electric backwardness of our country arose wholly from local obstruction—the obstruction of local authorities. Now, I propose to inquire if that charge is well founded. I am not here in any sense as a champion of officialism, and nobody has been more anxious for a long time past than I have been to see a wide development of electrical transmission of power. But there is no reason why, because the almost Utopian dream that I and others suggested in 1879, and put twenty-two years ago before some thousands of workmen in Sheffield—a dream that "each workman would have electrical energy laid on at his hand, would have transmitted to him, just at the time he might require it, a small amount of energy, at say a halfpenny per hour per horse-power," which he would be able to turn off like gas when he did not require it—has not yet been realised, that we should blind ourselves to the real cause of our position. Let us take as an example the Metropolitan Railway, some considerable portion of which I saw being constructed nearly forty years ago. Why are the trains not being run by electricity? Is it because of some restriction imposed? Is not the stagnation rather due to the lethargy of the chairman and directors of a private company, the sort of company that we were assured last Thursday would rejuvenate Great Britain? Why, the Bill to sanction running the Inner Circle electrically became an Act of Parliament four years ago—and yet, and yet! Now take another example—they are endless. Going the other day through a large electrical engineering works, I saw being made the iron cores of field magnets. They were being made from cast-steel obtained from Germany, and were being tooled by machinery obtained from America. Was that because of some restriction by some local authority? Was it not because the owner of that factory in England thought, rightly or wrongly, that he could get better cast-steel for field magnets in Germany, and better machines to work it with in America than he could in his own country? Coming to this question of distribution of electrical power, I ask you, Is it a fact that in those districts which are under the control of a Municipality electric energy is always dear, and in other places, where private companies rule, that it is cheap? Mr. Swinton asks me what that has to do with it. I will take an example, because examples are very valuable. A certain local authority wished to light its own borough electrically—a small borough. They got out plans for a boiler-house, engines, dynamos, etc. But within two and three-quarter miles of the boundary of their district a very large generating station was being put up by a very important private company. This seemed to me a favourable opportunity to carry out the idea of transmission of electrical energy. So when I was consulted, I suggested to the local authority, not to lay the foundation-stone of a central station, but to let me arrange for them with the great central station, one hundred times as big as theirs could be, to send the electrical energy "in bulk" (as it was called) to the boundary of their district. The Local Authority accepted my advice, and asked me to carry out the negotiations! There was no difficulty about way-

leaves; there was no difficulty of any kind; all of us were the most excellent friends. I informed the private company that they might use any system of transmission they liked, either one, two, or three phase, and transmit at any pressure they wished, the only stipulation was that at the boundary of the district they should transform it into two hundred volts direct current. Only a small amount was required to start with—some 500 H.P. Still, as we only wanted it at one place, and the local authority was to do all the distribution and the collection of the bills for electric lighting, as far as the private company was concerned it was exactly as if the local authority was a factory requiring 500 H.P. at one particular place. What was the result? It was not a question of want of legal power. The Company said that they had gone into the question carefully, and frankly assured me that even if we made a contract for ten years to take the whole supply of energy that the local authority might require, whatever it should grow to, they could not undertake to deliver it to us at the boundary of our district at under 3d. a Board of Trade unit. Of course we replied that we could not take the current, because it was cheaper to cart coal into the district and put up a generating station. That leads me to the very important question of the relative cost of carting coal and transmitting the energy through cables. With water-power it is quite different. In a place where there is no coal, and there is water-power, you can only transmit power, if you want to use that power, by the use of electrical methods or other methods, such as compressed air, or whatever you like. For it is hopeless to carry the water in waggons as the Great Eastern Railway brings sea-water, because if you do so you leave the energy behind. But in the case of coal, there is another way of transmitting energy, and that is in waggons. I won't trouble you with figures, but I have got wholesale estimates, tenders, for the carriage of coal from all parts of this country to other parts, and speaking as an electrical engineer I am extremely sorry to see the prices come out so low. It seems to be a fact that it is cheaper to carry the coal than to transmit the energy electrically, for say twenty miles, if underground cables and a maximum P.D. of 10,000 volts are allowable. Professor Ayrton.

Mr. C. B. CLAY: I am told I am a bold man to speak at this Institution, especially on a subject on which I am afraid I am not as well qualified to speak as most members; but I think that the experience that has been obtained in one branch of the electrical profession may be worth recording in a meeting such as this. At the last meeting of the Institution the position of the electrical profession with regard to legislation and to municipal competition and obstruction was dealt with by several speakers. First, with regard to Government obstructions, I should like to read two or three short extracts from an article which appeared in the *Times* of the 13th of June, 1884, and to call your attention specially to the moral which is drawn by the writer at the end of this leading article. The *Times* says: "It appears . . . that the action of the Post Office has been so directed as to throw every possible difficulty in the way of the development of the telephone and of its constant employment by the public. We say advisedly 'every possible difficulty, Mr. Clay.

Mr. Clay. because the regulations under which licences have been granted to the telephone companies are, in many respects, as completely prohibitory as an absolute refusal of them. . . . It appears that the telephones can only be used under restrictions which are as absurd as they are vexatious." And, further on, it says: "The conduct of the Office, although not legally dishonest, is at least morally indefensible. There can be no just ground for a claim to possess the telephone by virtue of words introduced into an Act of Parliament before the telephone was thought of; and the effects of this claim are nearly as disastrous to the public as to the inventors and owners of the instruments. . . . It is much to be wished that Parliament could find time to liberate the telephone from the bonds of red tape in which it is being strangled, and to allow its future to be shaped by the operation of the ordinary laws of political economy. In the meanwhile, and even when this desirable release has been effected, it will be prudent not to lose sight of the moral of the story. This is, if anything, that the practical applications of science cannot be safely or prosperously committed to the hands of a Government framed upon the model of our own." Mr. Raworth pointed out at the last meeting that companies had nothing whatever to fear from municipal competition on equal terms. I cordially agree with that. I have been fighting the opposition for many years, and I have never yet been beaten on equal terms, and I have never been beaten on unequal terms—very unequal terms in many cases. The whole point of the matter is thus equality. I propose to show that the same spirit which prevailed sixteen years ago, when this article was written, appears to be ruling our Government now. The history of the telephone branch of electrical engineering is a history of one long fight by the companies to be allowed to serve the public. In looking over some old papers, it seemed to me almost impossible to believe that at one time we were allowed to open call-offices at which a member of the public might go to speak to any subscriber, but one of the conditions on which we were allowed to do that was that we must charge a shilling, of which one-half was to go to the Government. It is needless to say what was the result of that. Then another thing with regard to trunk-working, communication over the trunk-wires was only allowed to subscribers at both ends, and then each must be a subscriber to the trunk-wire. For instance, taking from London to Brighton, a distance of about fifty miles, the charge fixed by the Department was £25; the subscribers in London paid £20 and the subscribers in Brighton £12. Therefore, to have any useful communication between Brighton and London, the subscribers between them had to pay £82—a rental of no less than ten shillings for each mile of wire. Then the company wanted to connect up post-offices, so that subscribers could forward telegrams. The Post Office, however, although they gave this facility to their own subscribers free, imposed a charge upon the company of five guineas per subscriber, which of course killed the business. This charge was eventually done away with, and for some years connection with post-offices was allowed. It has very recently been refused in the case of one of the London Exchanges, so that the subscribers are not allowed the facility on any terms. With

reference to the competition with the companies, Mr. Fawcett stated: "When that competition has gone on for some time, the Government would be able to judge who did the telephone business best. If it were done better by the private companies than by the Post Office, the Department would be delighted to have the whole telephone business of the country conducted by private enterprise. If, on the other hand, it was proved that the business was better done by the Government than by the private companies, the Government would have beaten the companies in the fair field of competition and they could keep the ground without any question of purchase arising." I need not ask any one to say whether, during the fifteen or sixteen years since then, the company or the Government has succeeded. I think it is pretty obvious. Mr. Clay.

General WEBBER: May I interrupt on a point of order? The gentleman is speaking to something which is outside the paper that we are discussing. It is painful to me to sit and listen to statements on the question of the telephone industry in this country, every one of which I think I could refute. Therefore I ask as a point of order that the speaker, for whom I have the greatest respect, be asked to adhere to the subject of the paper. General Webber.

Mr. CLAY: I think the title of the paper is a very wide one. It is "Before and After." I am dealing with the "Before." I submit that this is a case of a branch of the electric industry which is being hampered in a way in which it ought not to be hampered; and I think, in giving you certain information which cannot be at the disposal of many of the members, it may be useful and borne in mind in dealing with a similar problem connected with other branches of the profession. Mr. Clay.

I have told you that some years ago the Post Office allowed us to give our subscribers connection with the telegraph offices, so that they could deliver their messages. Now, within the last week or two, we are refused the facility for one of our exchanges. We were allowed to have it on payment of five guineas before; we are not allowed to have it on any terms now. Also, the facilities which subscribers enjoyed, and which some of them very much appreciated, of telegrams being transmitted to them, is now withdrawn from new subscribers. If the telephones were fully developed, surely telephones in connection with express messages would be a most valuable facility to everybody; but the companies are, of course, restricted from doing the business. The Post Office undertakes to do this business itself; but what does it do? It provides in London something like thirty-two places from which express messages can be sent. Under those conditions it is absolutely hopeless. If anybody wants to send an express message, he has to ascertain if the address he wants is within a mile of one of the post-offices. With regard to the veto of municipal authorities. In London, the County Council has the power to allow the company to place their wires underground. Some few years ago the company entered into an agreement with the County Council, and a resolution was passed by the Council enabling the company to place their wires underground. Nothing remained to be done but to put the terms into a formal agreement, and then the work would be completed. The

Mr. Clay. County Council kept the company waiting a year, and when they submitted the formal agreement they put in two absolutely new clauses which had never been mentioned before. Consequently the company were unable to accept them, with the result that now there is comparatively only a small amount of underground work in the County of London. That is a result which I say is most prejudicial to the interests of the public. I would simply urge on the Institution the necessity of doing everything that is possible, by committees or otherwise, to protect the profession from the obstructions of municipal authorities. So far as competing with municipal authorities is concerned, if they will allow competition on fair terms, I have nothing to say. I will meet them with pleasure, provided there is a fair fight.

[*Communicated*]: Two examples from many of municipal obstruction many be interesting:—Underground wires exist from Hammersmith, etc., to north side of Holborn. Local authority refuse consent to connect exchange on other side, distance not more than 30 or 40 yards. Underground wires exist from Croydon, Sydenham, Bromley, Beckenham, Greenwich, etc., etc., to a point near St. George's Church, Borough, and from a point about 90 yards distant to the City. Authorities refuse to permit connection underground for this short distance except on prohibitive terms.

General C. E. WEBBER: This Institution, before the Parliamentary Committee sat which passed the Telephone Bill of 1899, did the very thing which the last speaker suggests they omitted to do. That Bill practically freed the telephone by enabling the Post Office to grant licences far freer and far more in the interests of the industry than anything I know of in connection with Electric Light Provisional Orders. It is the charter of all those who wish to extend the use of and to popularise the telephone industry in this country.

Mr. Gavey Mr. J. GAVEY: The debate has, to my mind, rather digressed from the subject of the paper, and I would not intervene at this stage but that Mr. Clay has referred to what I must consider very ancient history in dealing with the telephone question. Perhaps unconsciously, he has done it in such a way as to lead members of the Institution to believe that the hampering regulations on the question of rates to which he refers exist at the present moment. Now trunk rates, whatever they may have been in the very early days of the industry, when we were all learning the business and before anybody knew how the thing was going to work out and what rates we could afford to charge, might have been high; but for many years the Telephone Company have had an absolutely free hand to charge exactly what they liked. As an illustration I may say that in many cases where the Post Office was in opposition to the Telephone Company in Provincial districts, they have gone to the Post Office subscribers and have provided, or offered to provide, wires and telephones free for considerable periods, in order to induce them o leave the Post Office service. I think in the face of that it is not ght for a member to come here and say the Post Office has prevented e Company from charging reasonable rates, and that the Department is acted unfairly and has prevented the public from receiving a cheap and proper telephone service. Again, on certain other points it must

be borne in mind that in acting as the Post Office has, it is really protecting the rights of the Government under the Telegraph Acts. Those Acts were passed as the result of a public demand pressed on the Government, and they were carried by Parliament in response to those demands. Any subsequent action, whether the action of the Committee of the House of Commons, of the House of Commons, or of the Post Office itself, is the result, generally speaking, of public pressure. If Mr. Clay or members of the Institution will turn back to the minutes of evidence taken before the Committees of the House of Commons, they will find the general evidence was in favour of the telephones being taken over by the State; and, therefore, it is not with the view of oppressing a private company that the policy complained of has been initiated, but to meet the demands of the public. Mr. Gavey.

Col. R. E. CROMPTON: I, in common with everybody, have not only heard the paper, but have read it more than once with very great interest. I put this question to myself as a question that concerns every one who has the interest and honour of the electrical engineering profession at heart as an English Engineer. Why are we in England supposed to be—I say advisedly supposed to be—behind America and the Continent of Europe, in the development of electrical engineering? The question is a very vital one for this Institution, comprising as it does practically the whole of the members of our profession who are of any note or standing. On reading through the paper very carefully I found myself in full sympathy with the author. I agree with him, that it is not on account of our inefficiency as engineers that we are so behindhand; it is due to external causes. And one of those causes no doubt dates from the 1870 Tramways Act—an Act which fettered private enterprise and enabled municipalities to compete with private enterprise by funds raised on the security of the rates. The Tramways Act was the commencement of the curse that has hung over the country ever since. It is useless for Mr. Hammond to pose as a man with an inconveniently short memory: he forgets how, after the Electric Lighting Act came in force, municipal authorities obtained scores of Provisional Orders which have never been put in force. The municipal authorities never moved until private enterprise showed them how electric lighting could be made profitable in this country. There never would have been fourteen pence, not to say fourteen millions, put into it by the public bodies, if it had not been that a few of us got together private capital to show that we in England could do what they were already doing in America. It is well known to many what a hard task that was, how the few thousands that were got together in the early eighties were obtained by begging a few enterprising men to find money in order to show that electricity could be provided from house to house. There is no doubt that at that time the municipal authorities were our enemies. You can all remember their stock phrase, "We must have control of our own streets," was always put forward if we asked to do anything in the way of connecting up houses. The result was that the Electric Lighting Act of 1882 was passed, and the ridiculous B. of T. licences granted. In spite of all these things we started a few private enterprises for electrical distribution, and carried them on to such a Colonel Crompton.

Colonel Crompton.

pitch that public pressure was brought to bear on the Government, so that the Act was altered a few years later. As a prominent actor in those events, I think I can speak with some authority, and it appears to me that we are now exactly in the same position as regards the distribution of power on a large scale all over the country as we were then with the electric lighting. It is, as Mr. Hammond very rightly says, quite a question of whether, under English conditions of demand for power, it can be made to pay; but, if private enterprise proves this, will not the same thing be repeated? Will not the municipal authorities hang back until they see somebody else pulling the chestnuts out of the fire for them? That is what they are trying to do, and assuredly they will then do everything in their power to keep private capital from being put into these power concerns, just as they did in the early days of electric lighting. But, in spite of them, these power concerns will be brought forward, and in a few years the Local Authorities will go for Acts of Parliament to extend their districts, so that they may be able to work big power schemes themselves. I am quite convinced that that will be done, for history repeats itself. I have said enough on that point, but I wish to call attention to another very important one. There is no doubt that the impression prevails among the general public of England that we electrical engineers are to blame for the backwardness of electrical progress in this country—a most shameful and a most unmerited imputation, when you consider what we English electrical engineers have done. I claim for England a large share of the great things that have made electrical distribution possible. When we mention Dr. Hopkinson, and remember what has been done to introduce accumulators, which were first made and used on a large scale as an aid to central station work in this country and afterwards copied by other countries; when we know that it was England, through Willans, that introduced direct-acting high-pressure engines, which made possible all early stations in crowded areas, if I omit the multiphase transmission business, what have other countries done that we have not done in England? Who is to blame for the fact that our daily papers accept the rubbish that any American or German tout can stuff into them? There is no doubt that the English public, instructed by its non-technical Press, thoroughly believe that we English electrical engineers are woefully behindhand in electrical knowledge, and that it must look for aid from America or Germany for the hoped-for coming advances in electrical progress. I cannot help feeling that certain members of this Institution who speak at these meetings are to blame for this. How often do we hear men of scientific standing, although quite uninstructed in the real status and practical attainments of our English electrical manufacturers, hold forth to us on their return from their outings in America or on the Continent on our want of knowledge, and how necessary it is for us to go abroad to learn. This is pretty hard on those of us who have studied, from motives of the highest self-interest, the very problems on which they pass judgment so glibly. Surely we who have the same ·entific attainments and add to it a much fuller knowledge of the ·ical conditions of our business and of the requirements of our

customers, and of the other factors which determine the choice of electrical machinery best suited to our Home and Colonial trade, are better able to judge of what is good for us than those who only glance at the matter during the course of their annual holiday. It would be well if such speakers, who by their remarks mislead the public and thereby do us English engineers more harm than good, would in future help forward the cause of electrical progress by dwelling more forcibly on the matters so clearly brought forward by the author of this paper and which no one doubts to be the principal cause of our want of progress. Colonel Crompton.

Mr. H. Hirst (*communicated*): Like all previous speakers, I wish to congratulate Mr. Madgen on the happy choice of a paper which, judging from the discussion which has taken place so far, bids fair to be the starting-point of a crusade against legislative and other evils that impede the natural progress of cheap electric light supply throughout the country. It struck me, nobody had disputed we were behind other countries. It seems to be an admitted fact that municipal trading in electricity supply means splitting up the electricity supply of the country into small units, and is therefore an agent against cheap all-round supply throughout the country. Mr. Hirst.

Various causes have been given for this state of affairs. Mr. Madgen blames one-sided, grandmotherly legislation; another speaker suggests pandering too much to the democratic tendencies of the age; Mr. Swinton blames the construction of the Board of Trade; but has it occurred to anybody that a great portion of the wrong of which we complain lies with the electrical engineers themselves? These large schemes — these Power Bills—can only be carried out with some measure of success if electricity is generated and transmitted on principles that only a few years ago were pooh-poohed in this country by many of the leading electrical men. When one pointed to similar big enterprises on the Continent or in America, one was told within these very walls that multiphase transmission was not wanted unless it be for the purpose of utilising water-power which otherwise would run to waste. The idea that tramways with overhead wires would ever get a start in this country was ridiculed. We were going to have nothing unless we could get something better. Now, during the last year or two our eyes have been opened to the fact that if we do not wish to be left behind in the race we shall have to do as they did abroad, and now that the rush is coming on and the manufacturers of this country are scarcely ready to comply with the sudden demand, we forget our own shortcomings and are ready to kick at the Government. After all, the Government takes its keynote from the view of the most enlightened officials that we can put at its disposal, and if these officials see discord and disunion amongst the leading members of the profession on cardinal points, how can they be expected to anticipate the wants of the profession?

If only electrical engineers had known a few years ago what they wanted to-day, as they do now, I am sure means could have been found to alter the legislation, and I for one object to blame any particular Act for the present state of affairs. We all agree that the

Mr. Hirst

future industrial prosperity of this country depends to a very great extent on cheap electrical current. In fact, as the cry in the early fifties of the last century, around which Ministers formed their programmes, was for "cheap bread for the working man," so I should say the cry during the next fifty years, around which politicians will cluster their parties, can be very nearly expressed as "Electricity a penny per unit through the country," or thereabouts.

The success of every industry, with scarcely any exception, will depend on this cheap commodity. To achieve this, more than the alteration of one or two Tramway or Lighting Acts will have to be considered, and if Mr. Sellon's suggestion and hope that the outcome of this discussion may be an organised attempt to achieve something and to tell the Government what we want, becomes an accomplished fact, I trust such a committee may be chosen from this Institution as will treat the points in question from a large-minded point of view, that will bring wide knowledge to bear on the matter, and treat it in such a thorough and business-like manner, and form such proposals that the leading members of the Institution will agree to and submit to its rulings with confidence. If that can be done I am sure no Government will dare to defy the unanimous opinion of so powerful a scientific and industrial body as this Institution; but I rather think it would only too cheerfully take its guidance from us under such circumstances.

Mr. Baker.

Mr. C. A. BAKER (*communicated*): I am inclined to think that the point of view taken in this paper by the author is somewhat past date. Eight or ten years ago the conditions now pointed out most undoubtedly existed. The original mistake was obviously made by the Board of Trade in putting an entirely new enterprise in the hands of municipal authorities who dared not risk money borrowed on the security of the rates in experimental work. This necessitated a very slow development; in fact, development was not aimed at, but security, whilst other countries went ahead, and are now reaping the benefit of the early progress they were enabled to make. A little later the municipal authorities began to find that consulting electrical engineers were available, and these gentlemen have been largely employed in designing work and imposing conditions generally, again with a view to security rather than development. The only experimental development on a large scale that occurs to me at the moment as having been adopted in this country is the five-wire system at Manchester; the subsequent working of this system has not secured its popularity. Consulting engineers and manufacturers take different views as to the way work should be carried out, and no doubt the manufacturers of this country have suffered considerably by being placed so much under the control of consulting engineers through the instrumentality of municipal authorities.

At the present time there appears to be an abundance of work on hand. Many of the schemes which have been promoted by companies and received Parliamentary sanction are lying dormant, or the work in connection with them is proceeding very slowly; whilst, on the other hand, for several large schemes, where the work has been carried out, foreign machinery and material has had to be purchased in order to

get them to work in reasonable time. My conclusion, therefore, is that the Board of Trade and the Government Departments generally have sanctioned more schemes than our manufacturers are able to compete for. Mr. Baker.

Opposition to power schemes and other similar undertakings is often introduced not entirely through the local authorities or through the Board of Trade, or through Parliament, but owing to the keen competition amongst the numerous representatives of electrical engineers in all branches of the industry. Consequently, when a good scheme is proposed, it is not unfrequently met by an exactly similar scheme which is brought forward independently to cover practically the same area.

In connection with development, there is a point where our central station engineers still lag very much behind Continental and American practice. As long as twelve years ago, when I was acting as assistant engineer to the Italian Edison Company in Milan, we were using incandescent lamps consuming three watts per candle-power. It is still the practice in this country to use 4-watt lamps. A small calculation on these figures, allowing an ample sum for energy consumed by electric motors, arc lamps, etc., shows from the revenue received from electric supply during the last twelve months, that the public have paid considerably over a quarter of a million sterling in excess of the sum which should have been paid had the more efficient lamp been adopted. There are power schemes at the present time which would be glad of the offer of capital to this extent, and if, as is generally considered necessary, we have to make the best fight we can against gas-lighting undertakings, this point of the adoption of more efficient lamps should not be delayed.

Mr. A. B. Chatwood (*communicated*): It appears to me that the author of the paper, as well as the members to whose remarks I had the pleasure of listening, have evaded the point. If the paper deals with a subject worth our while to discuss—and I for one think it does—then let us face the problem before us and see whether some solution cannot be found. Mr. Chatwood.

Mr. Madgen gives two principal causes—silly legislation and official opposition. I think these two causes could have been given as one, since the second could not exist without the first. No one who was present and heard the course adopted by Southport—a course which cannot be considered other than idiotic—can for one moment doubt the existence of this second or subsidiary cause.

But is "silly legislation" a fundamental cause? I venture to think not. I, and probably all our members, will give the members of the Committee of the House which sat on these Bills credit for honesty, at any rate, if not for foresight and knowledge.

If we are to find the causes which have led to this silly legislation, let us compare our position with that of our colleagues in America when the industry in that country was just beginning its rapid advance. There we saw overhead wires, with their attendant risks to life, freely adopted, schemes which were ill-considered carried out on a large scale, the rights of property owners disregarded, and the "sacred" claims of vested interests set aside.

Mr. Chatwood.

We, on the contrary, have to face the natural consequences of the character which has been bred in us by the historical developments of our country during many centuries; we have to remember that our legislators hold life and property so sacred that they will not allow immature or experimental schemes to be carried out to the possible detriment or injury of the public.

Such, I think, is a fair review of the position. Our problem is not, therefore, that which Mr. Madgen puts, but is much more complex and difficult. The problem, it appears to me, is to settle first what we as electrical engineers desire, and, secondly, to induce Parliament, or a sufficient number of its members, to take our view.

We have been to blame, I think, in the past in that we have wrapped up our little bit of knowledge and experience and kept it in our offices, so that none should leak away without bringing its value to our coffers; and I am afraid that with some of us this knowledge and experience may have got a little rusty and out of date. Let us, therefore, do all we can in the future to teach the public, and more especially our legislators, that we can carry out our schemes without unduly risking life and without disregarding the rights of property, or, at any rate, without giving back with our right hand twice, nay, tenfold, as much as we take away with our left.

I conclude by suggesting that a committee be formed, not of members of this Institution only, but including one or two representatives of the legal profession, men who have had experience in Parliamentary procedure and have been in close relations with municipal officials, since, unless the views taken by municipal officials and others are represented, the researches of such a committee would be of little value.

Mr. Baillie.

Mr. G. H. BAILLIE (*communicated*): In connection with the criticisms we have heard of English legislature in electrical matters, it may be of interest to note how Italy has made more rapid progress than, perhaps, any other European country, under entirely different methods of procedure. There, the electric power laws are based on the principle that a municipality or a province should be allowed to do what it pleases on the property under its control. Thus, an electric lighting concession is granted solely and entirely by the municipality interested, and the Government merely reserves to itself the right of approving the details, which approval is such a formality, that, in the case of one electric lighting station, started by Mr. Woodhouse and myself, we had supplied light for three months before the ministerial decree arrived, giving us permission to start. In other interests than their own, however, municipalities have no power of obstruction, by reason of a useful clause in the electric light law, passed in 1894, which compels every proprietor, whether an individual or a municipality, to give way-leaves through his property to electrical mains. A power scheme, distributing over a large area, would depend on the Provincial Councils, who control the inter-urban roads; a municipality could not refuse permission to lay mains through its district, though it could refuse to allow any supply. In Italy, however, if the supply were good and cheap enough to make the inhabitants want it, they could practically compel the municipality to grant a concession for supply. Municipal supply

stations are very rare, probably because no town population will credit the municipal authorities with any desire but that of filling their own pockets; private enterprise, to a great extent German, has given Italy the enormous number of electric lighting stations now in operation, many of which supply villages so small and so poor, that a station running under Board of Trade rules would be financially impossible. Mr. Baillie.

I think a serious obstacle to the spread of electric light in England is the cost of house installations, which is so high as to be almost prohibitive to the poorer classes; in Italy, where the cost is less than half, even for good work, installations of 2, 3 and 4 lamps form a considerable part of a station's load. The system of wiring adopted is a much cheaper one, and would not be sanctioned in England; but still the houses do not burn, nor do the Insurance Companies fail.

Dr. Thompson's contention that water-power is no cheaper than steam-power, on account of the cost of the water-rights, does not hold in Italy, or I believe in Switzerland, as all rivers are Government property, and a concession costs only an annual sum of 3 francs per H.P. However, the capital cost of a water-power plant, and the fact that a long transmission is generally required, to bring the power where it is wanted, makes the advantage of water- over steam-power a very doubtful one, in the case of electric lighting with a small load factor, but a very decided one, in the case of an all-day power load.

Mr. EBENEZER HOWARD (*communicated*): Mr. Madgen in his paper dwelt upon the intimate connection between the development of electrical undertakings and the great housing problem. The only satisfactory way of dealing with the latter problem is to induce large bodies of work and of workers to establish themselves in new districts. This is what Mr. Madgen contends for. But this movement must be concerted; for if manufacturers leave congested centres in a concerted manner, they can secure for themselves and for their work-people far better results than by leaving singly. Mr. Madgen speaks of a comprehensive scheme for the generation of electrical energy as likely to attract manufacturers out of our crowded cities. But this system should be part of a still more comprehensive scheme. Factories need not only cheap motive power and light, but cheap sites, low rates, opportunities for necessary extensions, and railway facilities. They must be secured against interruptions of light and air; and above all must have, in contiguous or quickly-reached positions, healthy areas and healthy homes for their work-people. Start, then, a comprehensive system in which the supply of electrical energy for all purposes shall occupy no mean place. Plan out on some large and now scarcely-occupied estate a city which shall be abreast of modern ideas, and let the city grow to that plan as a house grows to the plan of its architect. Mr. Howard.

It should, surely, be possible to combine private and public enterprise, and to produce a great concrete result far higher than either could alone effect. It is practicable by private enterprise to purchase a large estate and to develop it in the public interest; to pay a fair rate of interest to those who find the necessary capital, and to give them besides an excellent security for their money. Then it is also practicable so to lay out the estate, and to offer such terms as to tenure and

Mr. Howard. otherwise, that all classes will co-operate; and such a city will be followed, of course, by others, thus drawing the people out of our overcrowded cities and solving at the same time the problem of rural depopulation. This is the project of the Garden City Association—a project with which should be combined a comprehensive system of electricity supply.

Mr. Madgen. Mr. Wm. L. Madgen, in reply, said: So far as time will admit I shall endeavour to deal with the chief points which have been referred to by the various speakers.

In the first place I did not move a resolution, but I have no doubt that the almost unanimous opinion which has been expressed will have due weight with the Council, in considering the advisability of appointing a Committee to advise upon the most effective means of dealing with the serious obstacles with which the electrical industry has to contend.

Had I moved such a resolution I should have adopted Mr. Hammond's suggested amendment, that a Committee be formed to consider engineering matters connected with power supply, but for the fact that they will more usefully occupy the open discussions of the Institution.

The Wallsend power station on the Tyne for Newcastle and the north bank of the river down to North Shields, the County of Durham main power station at Gateshead, and the first main power station for the North Metropolitan district, are now rapidly approaching the completion of their first stage, so that the new era of electric supply has actually commenced, and that of small separate generating stations in such districts is passing away.

The matter we have been considering is no sectional question between one branch of the industry and another. It must be to the advantage of all that the applications of electrical science should be freed from the absurd restrictions imposed by the governing bodies of this country.

I am sorry that the question of Municipal Trading has been dragged into the debate. Apparently it would not have done for this paper, which, I am gratified to find, has practically the unanimous support of the members, with the exception of Mr. Hammond, to have gone forth without some objections being associated with it on behalf of such an apostle of Municipal Trading.

Mr. Hammond attributed the backwardness of our electrical industry to the cheapness of gas. Apart from the fact that we are not talking of lighting alone, but also of *power* and of *traction*, can such an argument be regarded as serious when it is well known that the cost of gas is mainly governed by the price of coal, and in this country the price of coal is common to both forms of illuminant?

Dr. Silvanus Thompson alluded to the interesting question of the economic limit of size of a power station. As you may suppose, such matters have been very fully considered by those of us who were actively concerned with the Power Bills of last session, and a large amount of information was gathered together. It was of course impossible for me to compress this material within the limits of my

paper. I felt in preparing it that my first duty to the Institution was to indicate the steps which should be taken to remove the obstacles to these developments which have already been amply demonstrated in practice elsewhere. Mr. Madgen.

Some five years ago the critical limit, beyond which it would be as well to have two stations as one of larger size, was placed by a prominent writer at about 5,000 H.P., but now that single steam-alternators equal this figure it is seen to be absurdly low.

There is, as a rule, no gain in the boilers after a power-station has reached a few thousand horse-power capacity, and as station size increases the boilers are not made larger, but increased in number. With engines there is a continuing advantage as the size increases, both in efficiency, space occupied, and weight per horse-power; and, as the largest sizes so far used for electrical purposes are greatly exceeded by those of a similar type used in marine practice, special experimental work is not yet occasioned by increase in size, and the cost per horse-power continues to go down as the size increases. Alternator design is now so well understood that untried sizes can be built with confidence, but there is said to be no economy in first cost per horse-power of unusual sizes, as the special designs and construction outweigh the saving in material and workmanship.

An example of the smaller economies effected in construction is that one crane will serve an engine-room containing ten generators as well as one containing five.

However large the station, there is still a gain in the capital cost per horse-power installed due to concentration, as it is much cheaper to erect and to house duplicate plant at one point than to construct the equivalent at several points. As capital charges are the largest item in the cost of electrical energy, the most important saving due to centralisation is the greatly reduced capital cost per horse-power installed.

In regard to running expenses, even if the size has become so great that the concentration allows no increase in the size of units of generating plant, the duplication allows many economies of this nature that could not be attempted with a smaller equipment. This is owing to the fact that the complexity of these economies does not increase with their increased size, while the saving is proportional to the size. Many details, such as oil-circulating equipment, coal and ash-handling machinery mechanical draft, economisers, &c., are thus affected. The larger the station, also, the greater is the differentiation of labour, and in consequence the greater is the efficiency of that labour.

It is now regarded as a truism that the greater the centralisation of the power plant the better is the load factor. In a lighting system the peak of the load does not coincide in residential and business districts. In a traction system, pleasure resort lines may have their peak at one time and the lines serving business districts at another. Combined lighting, power distribution, and traction systems gain still more by the improvement in load factor due to the combination of different loads.

Thus it appears that increase in size causes a continued increase in economy, so far as the power-station itself is concerned. This increase undoubtedly grows smaller and smaller as the size continues to increase,

Mr. Madgen. but never reaches zero. At a certain point this gain is met by the disadvantage of long feeders, but with high-tension transmission it is not reached until the district is a very large one, sufficiently large in areas of a suitable character for stations of a power hitherto unheard of.

The above remarks are in part a paraphrase of some interesting notes which appeared in the *Electrical World* of New York, August 13, 1899.

Other considerations which come into the question may be indicated best by extracts from letters received in June of last year from our friend Mr. Gisbert Kapp, of Berlin, from Mr. Edgar, President of the Edison Electric Illuminating Company, of Boston, and from Mr. R. S. Hale, of Boston.

Mr. Kapp wrote: "The principles upon which you have been working have long ago been recognised as sound in Germany. In Silesia there is a steam-driven station supplying a large district with 10,000-volt 3-phase current. The Rheinfelden station, as you know, supplies into Switzerland and Germany over wide districts, and its power is fully taken up. In Berlin the old system of having town stations each for a limited area is being supplemented by outside stations and long distance transmission. In Schönweide, to the east of Berlin, there is a large 6,800-volt 3-phase station which supplies various parishes. A new station is now in construction in the north, and another is projected for the west. In all there are now on order eight steam 3-phasers of 3,000 kw. each. One parish south-east of Berlin, for which I am consulting engineer, has an offer for current from the large eastern station at low prices. I am just working out the plans for a municipal station,[1] and as far as I have been able to see at present the home-made current will come more expensive than the current from the large eastern station."

Mr. Edgar said: "This Company has had, for the past ten years, its principal lighting station upon the water front near the centre of distribution. A year or two ago it was foreseen that the facilities of this station would be completely required for the winter of 1900. It therefore became an important question as to whether this station should be enlarged or a station laid down in some other section of the city. After considering this question quite thoroughly, it was decided by all concerned that the proper action to take was to enlarge the existing station, and we are now building upon the same premises, and adjoining the old station, a power house of a capacity equal to double that of the old station. The entire plant is so designed that for all practical purposes it is one station; but at the same time, as a matter of precaution against steam-pipe explosions or troubles of that character, both the engine and boiler rooms are divided into two separate and distinct risks, while the switch-board is located in a structure entirely separate from the remainder of the station. The old station had a capacity of 6,500 H.P., while the new one is laid out for 15,600, making a grand

[1] Since this discussion I have been informed by Mr. Kapp that upon his advice the idea of the separate municipal generating station has been abandoned, and the current is now supplied direct from the main power station he refers to.

total of something over 20,000 H.P. when completed. We have therefore had to meet the same problem which you raise in your letter, and have decided it as I have described—viz., to locate on one premises practically all of the generating facilities of the Company, subdividing them into two or more fire risks by substantial walls, but getting all the benefit of concentration in the keeping down of operating expenses." Mr. Madgen.

Mr. Hale wrote: "I should say that the general idea here was that stations of 50,000 to 100,000 H.P. are more economical than smaller stations. My own investigations have shown that boiler plants of 3,000 to 4,000 H.P. had reached the point where no further economy was to be gained by increasing the size, but as regards engines, the use of engines of 5,000 H.P. makes it necessary to have very much larger stations so as to have a reserve unit without having too great a percentage of the capital lying idle. If we go to engines of 10,000 or 20,000 H.P. each, of course that will mean still larger stations, and, judging from the increase in size of engines in the last five or ten years, I should think it very probable that a 5,000 H.P. engine may be considered a small-sized unit before long."

Before the year is out fuller particulars will be available of the development of several steam-driven electric power stations of ten to twenty thousand horse power which will further demonstrate the folly of continuing in this country the era of a wasteful little toy station for each township.

One of the finest examples is that of the Metropolitan Street Railway Company of New York. In the relatively small area of twenty square miles this company has 217 miles of tramway track, 82 miles of which are now worked electrically, and much of the rest, including the celebrated Broadway cable line, is being "converted." The generating plant has been concentrated in a power station on the water front at 96th Street, and comprises no less than 45,000 kw., the large 3-phase plant units being of 3,500 kw. each. The electro-pneumatic switchgear is divided up in a manner analogous to a modern organ, and the required "tune" can be played on it, so to speak, from a central keyboard. There are many other points of great interest in this power station, and by the courtesy of the British Thomson-Houston Company I am enabled to put in—for the library—copy of a complete description with illustrations and dimensioned drawings.

The controversies between the adherents of theory and of practice in this Institution, and personal questions of priority of disclosure, have become a very stale convention to many of us, and there is a general feeling, I believe, that we might well have less of them. I am sadly conscious that any sympathy I may have had for Professor Ayrton on such occasions has been dissipated this evening. It appears that he has been among the back numbers, and has found that he said something or other in 1879 which we may, or may not, recognise as a broad claim to modern developments in electrical supply over large areas. Professor Ayrton then said that it cannot be the fault of Parliament or of the Local Authorities that the Metropolitan Railway trains are not run by electricity. True, but what are the circumstances of the Metropolitan Railway? It is in itself a monument to private enterprise, but

Mr. Madgen. the net earnings are very moderate, relatively to the public service it affords, and it is a grave question of finance whether the additional capital cost of making a radical change in the method of working would be an advantage to the proprietors. Moreover some three or four main lines have running powers over part of the circle, and arrangements in regard to them have to be considered. We should all like to see the change brought about, but it is no discredit to private enterprise that it should take heed what it is doing.

Professor Ayrton mentioned that on behalf of a local authority he had approached a large company to ask upon what terms it would supply in bulk to the extent of 500 H.P. The reply was 3d. per unit, and under the circumstances I am not at all surprised. Why should any one with a power station, selling retail over a large area, go out of his way to supply at a special rate to one wholesale consumer with such a small demand?

There appeared to be some question as to whether Mr. Clay was in order in bringing in the telephone question. I am aware that the company is not very popular to-day, and any whip appears to be good enough to scourge them with, but I do not suppose that those of us who are unfamiliar with telephone work can realise the difficulties under which they have been carrying on the service. The wonder is not so much that it has been done indifferent well, as that it has been done at all. Practically the whole of their wires are overhead, they have to get wayleaves where they can, and are liable to be victimised at every turn. Mr. Gavey reproached the telephone company with having resorted to the device of offering a free service in certain cases in order to compete with the Post Office. It is the Government which is responsible for any expedients to which the company may have been driven in order to preserve their business. It is the Government which is mainly responsible for the present chaotic condition of telephone work.

It is a great satisfaction to me that Colonel Crompton has appreciated the intention of this paper. After the E. L. Amendment Act of 1888 was passed it was due to his initiative that pioneer supply stations, which have served as an incentive to much that since has been done, were equipped at Kensington, Northampton, Southampton, Hove, and elsewhere, and he affords us an example of what can be done for an industry by individual enterprise.

My thanks are also due to the several other representative members who have supported the movement in favour of the Institution taking steps for the removal of the legislative and other difficulties which are retarding so very seriously the progress of electrical work in this country.

Dr. Silvanus Thompson concluded his remarks with a parable, and if you will allow me I shall do the same. In most domestic circles—certainly in the one with which I am best acquainted—there is a small member who wishes to know why you leave the house reasonably early, and why you do not return until sometimes unreasonably late. It is usual to tell this small person that it is for the purpose of bringing her bread and butter. Well, this is a bread-and-butter question. It

Mr. Madgen

is a question affecting the interests of this Institution as a whole. Surely it cannot be in the better interest of any consulting engineer, however "municipal" he may be, that the industry should be "held up" as it now is. If the means of progress are enfranchised, surely it will not be any impediment to municipal enterprise in the same direction.

It is to our interest to see to it that these legislative restrictions are removed. And I may put it higher than that—it is our duty. Members of Parliament, County Councillors, and other elected persons have at length discovered that electric traction is a means of relieving the congestion of population. Some of them, indeed, are beginning to quarrel as to which of them first suggested it! Personally I believe that electrical power distribution, by assisting the migration of industries into more open country, can do even more to relieve overcrowding. If this be so, if the greatest evil of our time, the overcrowding and the unhappy condition in which the working classes are living, can be alleviated by means of electrical power distribution and traction on a comprehensive scale, clearly then it is our duty to see that the path is cleared for us to carry out our mission.

The President.

The PRESIDENT: Gentlemen,—May I ask you to give a hearty vote of thanks to Mr. Madgen for his interesting paper?

Carried with acclamation.

The PRESIDENT announced that the scrutineers reported the following to have been duly elected:—

Members:

Major Andrew Bain, E.E., R.E.
James Ferguson.

Associates:

Robert Marshall Carr.
Alexander Charles Cramb.
Richard Bertram Leach.
Henry James Nisbett.
William Harold Spencer.
Christopher Wilson.
Ernest Wilson.

Students:

William J. Daly.
Thomas Abba Davies.
Walter William Symper.
Edward Garfield Taylor.
Norman Thirlby.
Francis Edward Wilkinson.

[illegible]

[illegible] Each [illegible] which at [illegible] The works are [illegible] and the cables [illegible] con- [illegible] iron [illegible] is to be [illegible] scattered throughout [illegible] be reduced by trans- [illegible] The network from [illegible] connected with its [illegible] interconnections when [illegible] is desirable in order to [illegible] It is designed that the loss on [illegible] shall be about 5 per cent., and on [illegible] 1 per cent. at most.

[illegible] must be cheap. This is secured by (1) as good a [illegible] as possible, (2) a large output, (3) raw materials, coal, [illegible] as cheap as possible, (4) first-class plant.

(1) The load-factor is not under the control of the suppliers, except to the extent that they cater for power distribution. This is one reason for working a triphase system of rather low periodicity. In the distribution of electrical energy for both power and light from the same mains the sectional area must be such as to keep the losses fairly low; but there is no real difficulty in working a power supply system on the electric light mains, as for example at Frankfort. Polyphase systems of distribution are a success on the Continent, as for example at Mainz, Manheim, and Strasburg. In Strasburg, for example, there is a three-phase system of 3,000 volts transformed to 100. There 900 k.w. are transformed to continuous current for the tramcar system. The same should be done in Dublin. In Strasburg they produced 1,300,000 units for power, 1,190,000 units for the trams, and 230,000 units utilised in three-phase motors and transformed on the consumers' premises. They particularly liked the three-phase motors as being very convenient for power production under a great variety of circumstances. It has been doubted whether it is possible to balance the voltage on the three wires of a triphase system satisfactorily when used for electric lighting; but it has been proved by actual experience that there is no serious difficulty in doing this. With regard to power, it is to be hoped that in Dublin, as on the Continent, there will grow up a large demand for power, and so produce a satisfactory load-factor.

(2) A large enough output is practically certain from experience in other cities where a satisfactory system of electric lighting has been installed.

(3) Command of cheap coal and water is secured by going to the Pigeon House, where coal can be delivered from shipboard and water for condensing pumped from the sea. In addition to these advantages the Pigeon House provides land cheap, room for extensions, no troublesome neighbours, and a distance from Dublin of only three miles.

(4) The plant to be installed is carefully specified to be of first-class design. It is to be divided into two sets of 500 k.w. and two of 1,000 k.w., and alternative tenders are asked for both compound and triple expansion engines. It is desirable to have some comparatively small units for work during the times of small demand. The specification of the engines and dynamos is drawn up in terms of steam consumption per kilowatt. The steam is to be superheated about 150 or 160 degrees. A 26-inch vacuum (with allowance for low barometer) will be demanded. It is required that the consumption shall not exceed 19 lbs. of steam per k.w. in the compound, nor 17 lbs. per k.w. in the triple engines, which works out as 12·11 I.H.P. for the double and 10·84 I.H.P. for the triple expansion engines at an 85 per cent. all-round efficiency. The transformer loss need not be great, if we are to judge by experience elsewhere. For example, at Leeds, with a high-pressure alternating system, the average cost per unit in 1898 was 1·29d. generated and distributed to consumers, which may be compared with Bradford, which has a low-pressure supply for the same period at 1·43d. per unit. Of 29 works which produce at a lower cost than 2d. per unit, 14 are alternating and 15 continuous current, so that there is no difficulty about transformer losses. He considered that it should be

possible to generate at 1·5d. per unit in Dublin. This would be at 3·37d. with repayment, and if they charged 4·5d. for private and 2d. for public lighting this would provide 3·42d., and thus leave a little for contingencies.

(III.) He arranged to satisfy the ratepayers in the three ways enumerated.

(*a*) By not embarking on too large a capital expenditure. This was an important reason why he designed this triphase system. Low-pressure continuous-current systems are too expensive in feeders. By going to the Pigeon House, as already explained, a small capital expenditure on ground was secured.

(*b*) He proposed to utilise the existing alternating-current system by transformers at Fleet Street. The frequency would be reduced, but otherwise the present network, the five transformer stations, etc., would be utilised.

(*c*) To prevent disturbance of the streets he proposed to lay down immediately sufficiently large mains to supply extension for several years to come, and to lay the mains in such a sound and secure way that there was practically no danger of faults occurring in the streets necessitating their being opened for repairs.

(*d*) To provide efficient public lighting he proposed substituting for the present arrangements a system of 500 arc lamps, utilising the existing culverts where possible. These public arc lamps are to be on special circuits separate from the other power and light distributing system.

In conclusion Mr. Hammond defended the general principle of a three-wire system of distribution. If out of balance to the extent of 50 per cent., this would not produce a greater difference of voltage than 100 volts, which was only 2 per cent. on the 5,000 volts they were installing. The Midland Company, one of the largest schemes recently proposed, was providing a balanced three-phase system, and he might say that three-phase systems were almost universally employed now with success.

Mr Porte. Mr. A. E. PORTE congratulated the Section on getting Mr. Hammond to come over and unfold his scheme. He raised questions as to the difficulty of getting good foundations at the Pigeon House, and quoted some results of borings. He thought it would be interesting to hear how this difficulty was to be met. If the strata are as described, there would appear to be danger of the clay being worked out by the action of the tides.

He criticised the provision for motor load, which he thought was very inadequate. He would have been glad to have seen 600 to 800 H.P. provided for this purpose. In Cork, with a population about one-third that of Dublin, there was, eighteen months after the opening of the station, a motor-load of 400 H.P.

He desired to know why single-phase motors were not more used at present on the existing supply. He considered the provision of 500 arcs as greatly in excess of what could be required. He agreed as to the desirability of specifying 1,000 k.w. units driven by slow-speed

Corliss compound engines, but wished to know what efficiency was expected from the triple engines on a low load. Mr. Porte.

Mr. R. HUMPHRIES spoke as to the effect of the voltage of a three-phase distribution system when a large inductive load such as an induction motor was thrown in and out of circuit. He had had experience on the Dublin Southern Tramways with three-phase transmission, and knew the variation to be considerable that takes place on the terminals at the far end of a cable. The General Electric Company of America had developed a system known as the Monocyclic System for just this purpose, to keep the motors and lamps on circuit separated from one another. The conditions were altogether different to throwing continuous-current motors in and out of circuit. The makers were guaranteeing a maximum variation in voltage with change of load, but did this refer to an inductive load? Mr. Humphries.

Referring to Mr. Hammond's wish for a large load-factor, this was exactly what was offered by a combined traction and lighting system. Notwithstanding Mr. Hammond's reference for plant made to order, he preferred the system of having standard plant, as it could be produced of good type much cheaper than special plant, and the fact that it was standardised proved that the machinery had well withstood its tests.

Mr. C. P. C. CUMMINS also expressed a decided opinion in favour of specifying plant of standard pattern. He had had experience in America of the value of standard patterns, and had been strongly impressed with their value. He regretted that Mr. Hammond had been unable to give some of the figures which so staggered him, when considering other systems. Mr. Cummins.

Mr. S. A. MALPAS asked whether any difficulty was experienced in getting a sufficient number of competitive tenders, as other efficiencies were introduced such as air-pump and condenser efficiencies, and many of the smaller manufacturers would decline to tender rather than take the responsibility. Mr. Malpas

Mr. SAYER questioned the facilities at the Pigeon House for delivery of coal, as large vessels could not there come alongside. He also desired information as to the facilities for obtaining fresh water for the boilers. Mr. Sayer.

Mr. J. R. SYKES reiterated Mr. Sayer's doubt as to the cheap delivery of coal, and further inquired whether the roadway was sufficiently well constructed to carry heavy machinery, and asked if a carrying company had not raised difficulties as to the delivery of plant. Mr. Sykes.

Professor G. F. FITZGERALD (*Chairman*), in calling on Mr. Hammond to reply, congratulated the Section on having had this interesting and valuable communication brought before it, and particularly thanked Mr. Hammond for the very great amount of trouble he had taken in giving them this account of the proposed Dublin supply. He expressed a preference for clothes made to order rather than for ready-made clothes, and rather sympathised with Mr. Hammond's views as to the desirability of designing plant for the special purpose they were to fulfil. He called attention to the value of the work done by scientific investigation, in that the two quantities most accurately and easily Professor Fitzgerald.

Professor Fitzgerald. measured were water and electric energy; the oldest and the newest of measured things—matter and energy. He expressed a hope that a three-phase system would be introduced, because the educational value of such a complicated system, and one whose working was so difficult to understand, would be very great, and it would emphasise in Dublin the necessity for a sound scientific basis for the education of engineers.

Mr. Hammond. Mr. HAMMOND replied that as regards the foundation the Corporation were acting under the very best advice, and that the citizens might be confident that no structures or engines would be erected except on perfectly secure foundations, and that there was no serious reason to think that the expense of foundations would be excessive. In answer to the objection that the installation was not big enough, he could only hope that it would be much bigger, and that while the Tramway Company desired to obtain the supply of power for lighting Dublin, the Corporation were desirous of obtaining the supply of power to the tramways. One of the reasons why single-phase motors were not more used in Dublin was on account of the high frequency of the present supply; with the lower frequency proposed he expected a considerable development of their use. He did not think they were proposing too great a number of arc lights for a city like Dublin, and the power involved was so insignificant that it was undesirable to restrict the number below what might be of service. There had been no difficulty at all in getting contractors to undertake to deliver heavy goods at the Pigeon House, which, notwithstanding the various criticisms was, in every way suitable for an installation, being nearer the distributing area than most new installations in large cities, which were gradually being moved outside the cities for the sake of space and other facilities. In conclusion he expressed the pleasure it had given him to make this communication to the Dublin Section.

A cordial vote of thanks to Mr. Hammond for his exceedingly interesting paper was afterwards carried by acclamation.

NEWCASTLE LOCAL SECTION.

Paper read at Meeting of Section, January 14, 1901.

ELECTRICALLY-DRIVEN MACHINE TOOLS, AND THEIR ADVANTAGES FOR USE IN ENGINEERING WORKSHOPS.

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By G. RALPH, Associate-Member.

Although the subject chosen is of great importance, particularly in great engineering centres like Newcastle-on-Tyne, the author feels that an apology is due to the Local Section of the Institution of Electrical Engineers for the *character* of the paper, since there is little or nothing in it which is not already well known to many.

Owing, however, to the list of papers for the Session being somewhat scanty, it was necessary for some one to make an offer, and the author hopes that his attempt may be the means of inducing others to produce something better.

Before describing the methods of gearing and application of electric motors to various purposes in the engineering shops, it may be well to draw attention to some of the points which appeal most strongly to a works manager in favour of electrical driving.

One of the foremost of these is the ease with which machines may be so arranged with reference to one another as to facilitate the "following on" of work from one tool to another, without an excessive amount of handling, either by manual labour or by overhead cranes. Where all the machine tools are driven from line shafting, they must follow a more or less rigid arrangement in lines parallel to the shafting, which is not by any means the most convenient way in many cases, although this arrangement has become so general from years of use that it does not at once strike the casual observer that it is inconvenient.

Many shops are built in two bays, only one of which is served by overhead travelling cranes, while the main shafting is erected in the other bay, and thus most of the machines are collected together in or near that bay. Where the height

of the shops is limited, as is often the case when the building has two or more stories, it is impracticable, in many instances, to place machines on the far side of the bay served by the travelling cranes, owing to the difficulty of getting belts across from the line shafting. The author is, of course, aware that this difficulty does not exist in large shops where there is plenty of head room, as a second line shaft can be erected below the traveller gantry on the walls or columns. But one cannot make more head room if it does not exist, and has to make the best of circumstances. In such cases the application of electric motors to individual machines proves of great advantage.

Taking a case which came under the author's notice some time ago as an instance. It used to be necessary to transport all the heavy magnet-castings from the planing-machines at one end of the shop by hand travelling cranes, to a large radial drill at the opposite end. After drilling, these had to be brought back again to the boring machines near the place where they started from. It was decided to move the radial drill from that position to a point about midway between the planing and boring machines on the opposite side of the bay. These large masses of material now only have to be lifted across to the other side of the bay for drilling, and zig-zag back again for boring, instead of being twice carried almost the full length of the shop. This instance gives some idea of the convenience of being able to group machines together in the best manner. It would have been impracticable to drive this radial drill from the line shafting in its new position in this particular case. Several other tools were moved to new positions from the same considerations, and this resulted in much less handling of material and in greater output. Those of us who are engaged in the manufacture of heavy machinery will know that the proper supply and following on of work from one machine to another is second only in importance to the output of the machine tools, from a commercial point of view. As it has been very aptly put in an article by Mr. Hutton, in one of the engineering magazines, "the productive shop, being a tool, can be regarded as a *machine*, to be designed for a specific purpose. In this design the special tool is the *cutting edge*, while the internal system of transportation, whereby work is brought and presented to

the special tools, may be regarded as the *feed motions* of the machine."

Another great advantage of applying separate motors to individual tools is the ease with which the speed of the machine can be varied without the necessity of shifting belts on the cone pulleys. The average machine-hand will, in most cases, not take the trouble to shift his belt to suit the size of work unless closely and constantly watched by the foreman; but give him a small regulating switch close to his hand, when he has only to give a knob a quarter turn to increase his speed by 25 or 50 per cent., and he will do it.

Many motors for machine tool driving are so constructed that the speed may be increased to double the normal speed simply by varying the exciting current in the shunt winding, which is, of course, much more efficient than inserting resistance in the main circuit for varying the speed. If, in addition to this, the motor-armature be double-wound with two commutators, a variation of 1 : 4 or more can be obtained, since, by connecting the windings in series or parallel, the speed can be halved or doubled as desired. Also the graduations of speed can be made much finer than by step cones and back gear, so that it is possible to get the correct speed for almost any variety of work put on the machine. It is quite a common thing to have 15 or 20 steps on the regulating switch, and an equivalent number of different speeds. While speaking of this matter, it may be mentioned that machine tools might in many cases be much improved in this respect for ordinary belt driving; the graduations of speed on many lathes, &c., are not satisfactory, and the machine tool makers seem to have paid very little attention to this point in the past, although it is receiving more now.

The possibility of a wide range of speed is, in the author's opinion, a very strong argument in favour of continuous current motors in engineering works as against alternating current motors. With single phase motors, whether synchronous or self-starting induction motors, it is not practicable to vary the speed, since both classes depend more or less on running in step with the generating plant. With two- and three-phase motors the same objection exists, though not in the same degree. The speed can be varied by special devices, but the author understands that when

running below their normal speed they are very inefficient. He has no actual experience with them, and it will be very interesting probably to most of us if anyone can give further information on this point in the discussion. The author regrets that he has to confess his ignorance on this subject; but, according to President Perry's address, many other engineers are in the same lamentable state. This is not a paper on the relative merits of direct and alternating current supply for motors; but the question of speed variation is one which should not be lost sight of by engineers when debating which system to install in their workshops or shipyards.

Since writing this, the author has read the excellent paper given before the Manchester Section of the Institution, by Mr. H. A. Earle, on the relative advantages of direct and alternating current distribution, which states in a much better and clearer form than has hitherto been published the points for and against each system.

Another advantage of applying separate motors to individual machines is found when it becomes necessary to work overtime in the shops. It may so happen—and often does in the author's experience—that, although the shops in general are not overcrowded with work, a tremendous amount may have accumulated for some particular class or group of machines. It is obviously very uneconomical to run the whole of the shafting, belting, main engines and boilers for the sake of one or two machines till seven or nine o'clock every evening, but yet the work must be got through. One cannot purchase new lathes or boring machines at a few days' notice—in this country, at any rate—nor would it be advisable or profitable to do so, if the large increase in this particular class of work is merely temporary, as is generally the case. But if the works manager be fortunate enough to have these machines electrically driven by their own special motors, his difficulty is solved, for it is possible to work any amount of overtime without serious increase of establishment charges. More fortunate is he still if a supply of energy be available from a storage battery or from the town mains. It is then possible to run night and day by putting a double shift of men on, until the accumulation of work at these machines has been got rid of. It is even possible to take advantage of

a double shift on machines which are driven from the main shafting, as a rule, by putting down a temporary motor on the floor to drive the machine at night, while through the day it may be driven either from this motor or from the shafting as before.

Just now, at the works with which the author is connected, there is a large amount of work for which only two or three machines in the shops are well adapted, so two of these have been running night and day for some months past. One is a horizontal boring mill, which is always driven by its own motor; the other is a large face-plate lathe of the ordinary pattern, usually driven by belt from the shafting. At night, the belt is left on the loose pulley of its countershaft, and a belt is slipped on to the fast pulley from a portable motor—with spur reduction gearing attached—which is spiked down to the floor behind the lathe. The starting and regulating switches are temporarily fixed on the front of the lathe headstock, convenient to the man's hand. These machines and the necessary lights are run from the storage battery at present. Probably, before long, the continuous-current supply from the Newcastle Electric Supply Company will be available for this or any other purpose. Again, in the event of an urgent repair or breakdown, work must sometimes go on without intermission, and in such cases it is very convenient to be able to put down a motor at an hour's notice, to run any machine all night. A few weeks ago, a motor at one of the shipyards on the Tyne got accidentally damaged by water or otherwise. The field-magnet coils were brought to the works at 4 p.m. to be rewound, and were wanted as early as possible the next morning. Shortly after 5 p.m. the magnet-winding lathe was belted to a motor and ready for running all night, and the repairs effected by 6 o'clock next morning.

It often happens that contracts are taken for manufacturing machinery of a larger size than any of the regular tools in the shops are capable of dealing with. It is then a case of rigging up special appliances, and carrying them to the castings to be operated on. For instance, some large steel rings for the yokes of multipolar dynamos were far too large for any of the lathes or boring machines to deal with. So a boring bar was made, with standards easily adjustable for height; the magnet-rings were fixed to the slide-rails of the

[illegible] in most cases [illegible] statement is often rather pooh- [illegible] engineers, and said to be exaggerated. [illegible] our statements. The [illegible] the power itself can be measured [illegible] power for all the varying [illegible] is in itself an advan- [illegible] before the American Society of [illegible] Professor Benjamin stated that in [illegible] where heavy machine work was [illegible] average of 62 per cent. of the power produced was

used in driving the shafting. In one case the loss was 80 per cent.

The author has made some tests recently, the results of which are given below. A length of 100 ft. of 2½ in. diam. shafting, running at 150 revolutions per minute, to which twenty-four machines were belted, took to drive it light, 45 per cent. of the power necessary when all the machines were in use. In another case, the power taken to run the shafting and belting on the four floors of a works was 56 per cent. of the average power required when the shops were in full swing. The motor used for driving was belted to the centre of the shafting on the first floor. On the ground and first floors the shafting is 2½ in. diam., and a total length of 240 feet, running at about 130 revolutions per minute. To this shafting are belted 71 machines of all classes, the ground floor chiefly lathes and planers, and the first floor light brass-finishing machines chiefly. There is about 100 feet of 2 in. shafting on the second floor, running at 150 revolutions per minute, driving twenty-one machines, chiefly for brass-finishing work and magnet-winding; and a short length of 1½ in. shafting on the third floor. The loss of power in the line shafting and belting is going on continuously during working hours; whereas, with motors to individual tools, whatever loss there may be in the leads, motors, and reduction gearing, is not continuous, for the motors are stopped when the machine is not at work, through changing work, or setting fresh tools, &c. There is thus a further gain in this respect.

With regard to the total h.p. of motors installed, and the average power used, it is in most cases possible to put down a much greater collective h.p. in motors than the power station is capable of supplying, for it seldom happens that the whole of the motors are called upon to develop their full power simultaneously. The ratio seems to vary within pretty wide limits. It is stated that in the Baldwin Locomotive Works only 1,300 h.p. serves about 3,100 h.p. of motors, a ratio of 1 : 2·4. In the Bullock Electrical Manufacturing Company's Works it is stated that for 127 h.p. of motors and lighting installed, the average load for working hours is only 27 h.p., a ratio of 1 : 4·8. This is rather a different case, however, since the lighting is included. Probably a ratio of 1 : 2 will in most cases be

ample allowance for a shop using separate motors to each machine, the amount of power for lighting *not* being included.

Until electric motors came into use for driving machine tools, very hazy notions seemed to prevail as to how much power a machine of a certain class or size demanded, and those who were least well informed were frequently the makers of the machines. A somewhat striking instance of this may be mentioned. A special form of grinding machine, for trueing up the edges of armour-plates, was put down at some steel works. The manager asked how much power would be required, and the makers told him 6 h.p. He thought he would be on the safe side, so ordered a motor to develop 10 h.p. When this was put to work complaints were made to the electrical firm who made the motor that it would not do its work; and as this was their first experience of electrical driving, and they were contemplating putting in a big power plant, they were much perturbed, and did not think electric driving was up to much. The electrical firm was very anxious to reassure them on this point, and the author was sent down to investigate matters. It was found that the motor was supplied with current from a small engine and dynamo of about 25 or 30 h.p. used for lighting, and the grinding machine was demanding more energy than the engine was capable of developing, and practically pulling it up short every few minutes. This was explained and demonstrated to the directors and managers, and permission was given to couple the motor up to a much larger dynamo used for welding. It was then found that the grinding was done in a highly satisfactory manner, but the motor was absorbing nearer 35 or 40 h.p. than the 10 h.p. for which it was built. It continued to do this for some months, occasionally demanding even 60 h.p.—a striking testimony surely to the good qualities of the motor. This result was highly satisfactory to all parties concerned, as there is now a power station with four sets of 200 h.p. each, and a corresponding load of motors for driving machine tools of all classes.

Let us pass on to the various applications of electric power. Travelling cranes are now almost invariably driven electric motors, and every one agrees that this is an ideal d of applying power to this class of machinery. The

question of three-motor *versus* one-motor cranes has been much discussed. Possibly we shall have further discussion to-night. The balance of opinion seems to be in favour of three-motor cranes, though the one-motor type can be made to work quite as satisfactorily, in spite of what interested parties say. One firm of crane-makers who advocate the three-motor type to the exclusion of the other, built one of their earliest—if not the very first—cranes for a single-motor drive, and their experience with this has apparently been sufficient to change their views utterly. But the fault did not lie in the single-motor drive; it was the bad design and workmanship of the mechanical parts of the work—gearing, clutches, etc.—which caused the whole of the trouble. The author speaks feelingly, as this particular crane was a source of continual worry and trouble for over a year while under his control, until another firm practically rebuilt the crab, remodelled the gearing, etc.

If it be a case of putting in an entirely new crane, probably the three-motor type is advisable; but where an existing crane has to be altered from, say, a square-shaft drive, it is decidedly simpler and more economical to apply a single motor to drive the transverse shaft, and leave all the gearing and clutches as before. The author has had experience with several highly satisfactory examples of this arrangement. Liquid switches are a very convenient form of controlling gear for crane motors.

One or two somewhat interesting applications of motors to cranes have been made at Messrs. J. H. Holmes and Co.'s works. In the machine shop there are three Tangye hand travelling cranes in one bay. Since the time these were put up, the size of dynamos constructed has increased so enormously that it was a very slow business handling the heavy castings now dealt with. It was a somewhat difficult problem how to apply motors to the lifting motion, owing to the limited space between the crane girders, and the absence of head-room. However, a motor was suspended under the girders in a wrought-iron framing bolted to the crab. As this was of necessity somewhat lacking in rigidity, a spur or worm-gear reduction was out of the question. So an ordinary 1-inch pitch bicycle chain gear was used to transmit the power from the motor shaft to the hoisting gear. A stock size of sprocket wheel was screwed on th

motor shaft, and a steel disc about 18 inches diameter was cut in the milling machine to fit the chain, and this was fixed to the hoisting spindle.

A 5-ton traveller was first altered in this way, current being supplied to the motor through a flexible cable from a plug and socket on the wall near the works entrance, where most of the loading and unloading is done. It proved so successful, however, that wires were soon stretched along the shop in the usual way, so that the motor could pick up current, and the power lift be used from any position in the shop. A 3-ton crane has also been fitted with the same arrangement. The third crane in the bay has not been so fitted, and it is interesting to see how the shop labourers will always go and fetch one of these power-cranes from the other end of the shop if any heavy stuff has to be lifted, rather than use the hand-crane which happens to be close by. It should be made clear that electric power is only applied to the *lifting* of these cranes, and not to the travel and the traverse of the crab. The saving of time in lifting heavy weights is enormous. The motors are series wound, so the speed of lift automatically regulates itself according to the load to be lifted.

Another rather novel application of power has been made to a similar 3-ton hand crane on the floor above. It used to be a very slow, laborious job to lift heavy machinery from the ground floor to the first floor through a hatchway, the height of the lift being about 18 feet. So a motor was fixed down permanently on the floor at one end of the hatchway, and a cast-iron pocket wheel to fit the hand hauling chain was keyed to the motor shaft. The motor is fixed in such a position that when the hauling chain has been slipped over the pocket wheel, and the crab moved slightly across the shop to tighten the chain sufficiently, the crane hook is in the centre of the hatchway. By this means a load can be lifted from the ground floor in about two minutes, which formerly used to take four men about half an hour to do. Of course, in this instance the power lift is only applicable at this particular spot ; but, to enable loads to be moved about the shop, it is usually only necessary to raise them a few inches from the floor, which is not a long operation by hand. A similar arrangement has been fitted to a crane on the second floor also.

One very neat method of applying motors to machines is to suspend the motor on a hinged frame attached to the joists under the floor, and drive by a belt coming up through the floor. The weight of the motor keeps a tension on the belt, and, of course, with a large motor it must be partially counterbalanced to prevent too great a tension. Several machines in the pattern shop at Messrs. J. H. H. & Co.'s are driven in this manner, there being a basement below the shop. Circular saws and band saws are so driven, and a great deal of floor-space is thus saved.

There is a somewhat novel application of a motor in the pattern shop. It was necessary to provide a much larger lathe for turning up big magnet-ring patterns and similar things, and, as there was a five-horse power slow-speed motor available—the normal speed being 150 revolutions per minute—it was mounted on a cast-iron stool, and a disc flywheel attached to the armature shaft direct, to form a chuck or face-plate, to which work could be fixed in the usual way. Proper provision was made, of course, for taking up all end-play of the armature. Some dynamo slide rails were bolted to the floor in front of the motor, and a cast-iron pillar to support the T-rest can be moved in any direction on these rails to suit the work. This novel lathe will swing work up to about seven feet diameter, and has proved very successful. The speed can be varied from 150 to 600 revolutions per minute by shunt-resistance. An ordinary lathe to swing this diameter of work would have been an expensive matter. Also, it was possible to fix this close against the wall, thus taking up much less space than a lathe would occupy.

A radial drilling machine for drilling and tapping up to 2 in. diam. holes is driven by a two horse-power motor, the gearing being a Hans Renold silent driving chain. The motor is mounted on the cast-iron box containing the gear wheels of the machine at the back of the pillar, the distance between centres of motor and machine shafts being only about two feet. The starting and speed regulating switches are mounted on the pillar, close to the man's hand.

While speaking of drilling machines, mention should be made of the application of electric motors and flexible shafts. The motors are usually mounted with their reduction gear complete on wheels, so that they can be readily run about

from one place to another. The "Stow" flexible shafts are usually about eight feet long, and are coupled to the motor shaft by a universal joint coupling. The shaft may be curved and bent in any direction, and with a suitable drill press, holes up to 2 in. diam. can be drilled with a 1½ h.p. motor. The convenience of these outfits in shipbuilding, bridge, and building construction can only be realised after they have been employed. The current is conveyed to the motors by an armoured flexible cable, which may be of any length required. Smaller motors and flexible shafts for use with a breast drill press, for holes up to ⅜ in. diam. are also largely used. In many cases a self-fixing magnetic drill post can be used with advantage in place of the ordinary "swan-neck." These magnetic pillars or posts hold on with an astonishing force. The author tested one in drilling a hole 1½ in. diam. in cast iron, the magnet only taking rather less than 1 ampere at 120 volts. The weight of the post was about 50 lbs., and it required a direct pull of 16 cwt. to detach it from a rough machined steel surface.

Another very neat portable drilling tool was brought out a year or two ago by one of the leading electrical firms in the country, for drilling small holes rapidly in the thin deck plating of torpedo boats. This motor and reduction gear is enclosed in a metal case, with the drill projecting at the bottom. There are two handles, one at each side, on which the workman presses when drilling, and by which the machine is carried from point to point. It only weighs about 60 lbs., if the author remembers rightly. A test, made in one of the shipyards on the Clyde, proved that about 600 holes 3/16 in. diam. could be drilled through a plate ⅛ in. thick in one hour.

Perhaps driving a planing machine with a separate motor is about the most severe work to which a motor can be put, as the load at the moment of reversal with a great weight on the table runs up to three or more times the normal current. If a motor drives the planer by open and crossed belts, and runs continuously in the same direction, a heavy flywheel on the motor shaft is of great assistance in helping it over this sudden strain. In one case with which the author had to do, a very large vertical and horizontal planer—or wall planer, as it is sometimes called—was driven by a compound-wound motor, with the series helping the shunt, and thus

the considerably higher speed at which the motor runs at light load was of advantage in the return or non-cutting stroke.

Another case of power being under-estimated by the machine tool makers was in the case of a slab milling machine for milling surfaces up to 18 in. or 20 in. wide. For a specified depth of cut and rate of feed they estimated it would require 6 h.p. to drive it, whereas it was found that the power absorbed was nearer 15 h.p. Two motors were applied to this milling machine eventually, one to drive the cutter and the other—a smaller one—to drive the feed motion and table. In both cases the speed could be varied within very wide limits by means of resistance in the shunts. The feed and speed being entirely independent, was found of great advantage in this case. A comparison was made between a slab milling machine, using a fluted spiral cutter of the ordinary pattern for heavy work, and a machine which was really a cold sawing machine, but which was fitted with a milling or facing head, having inserted tools or cutters. It is interesting to note that the latter class of milling only absorbs about one-half the power for a given weight of material removed in a given time. This is due to the fact that the cuttings are much larger and thicker, similar to the cut of a lathe or planer, while the ordinary milling cutter removes the metal in very much finer particles.

Horizontal boring and drilling machines can very suitably have the motor mounted on the rising and falling head or saddle of the machine, and geared direct by spur gearing. A machine of this class has been so fitted recently. It was formerly driven from the main shafting and a countershaft. A jockey pulley and weight arrangement was necessary to keep tension on the driving belt, for, when the saddle of the machine was raised or lowered to suit the work, the length of the belt varied greatly, the drive being at an angle of about 60 degs. The application of the motor has dispensed with this unsightly arrangement, and has enabled the machine to be moved to a more convenient position at the opposite side of the bay.

It is sometimes more convenient for lifting work on and off certain classes of machines—say, horizontal lathes and radial drills—if they are fixed at right angles to the walls or columns. This is not usually very practicable with belt

driving from the shafting, but can be readily arranged when driven by separate motor. A boring mill was arranged in this manner recently, the countershaft being supported at one end by a hanger bearing under the travelling crane gantry and at the other end by a pedestal bearing fixed on the top of the boring mill. The motor was fixed on an angle bracket bolted to the side of the machine, and is belted to the countershaft by a short belt. The switches are arranged at the opposite side of the machine, near the feed handles, &c.

In the case of very large and comparatively slow running lathes and boring mills, a motor is sometimes coupled direct to one of the cone pulleys of the machine, and mounted on a bracket on the wall, to drive down to the cone pulley of the machine. Or, if a motor with very large range of speed be used, it is coupled direct to the lathe gearing, and so dispenses with the cone pulleys and belt altogether. Some very large lathes for ordnance work had hinged frames attached at the back of the headstock. A cone pulley corresponding to the lathe cone was fitted on the motor shaft, and the motor mounted on the hinged frame. The weight of the motor then kept the requisite tension on the belt, as it tended to fall away outwards from the headstock. A very short belt was sufficient, and the arrangement took up very little space. In connection with one of these large lathes, when first fitted with a motor the workman in charge was very sceptical about how much work he could turn out of his lathe. He asked how big a cut he might put on without pulling it up, and was told to put on all he could. He was turning a heavy gun forging of about 30 tons, and there were two saddles on the lathe with four tools in each. When he got them all taking their full cut he was very much astonished that it apparently made not the slightest difference to the motor, which, of course, ran as quietly and practically at the same speed as before. He was genuinely converted to electric driving.

A very convenient little tool can be made by mounting a small motor, say $\frac{1}{4}$ h.p., on a square steel bar, and attaching an emery wheel direct to the armature spindle. If any lathe centres have to be trued up, this apparatus can be bolted on the tool post of the saddle, and readily used to grind up the centre.

Nothing has been said about the relative merits of various forms of gearing, as this seems rather outside the scope of an electrical paper. The question as to which is best suited for a particular purpose depends largely on the circumstances of each case; but direct-coupling, belting, pitch chains, Hans Renold chains, spur worm and friction gearing, have all been used with very satisfactory results in electrically-driven machine tools. The author may mention that a most interesting friction gear for reducing speed has recently been brought out by a gentleman named Brown, and is called the "Twentieth Century Gear." It seems to have some excellent points for use in connection with small high-speed motors, though no figures are available yet as to its wearing qualities. It is an internal rim friction device with three rollers, two of which revolve on fixed spindles, while the other is free to change its position somewhat according to the torque. The low-speed friction driven rim or pulley is concentric with the motor spindle, and takes up no more space than an ordinary pulley would do. The efficiency seems to be remarkably high. Worm gearing is often employed where a very large reduction of speed is necessary, and in cases where it is important that motion cannot be transmitted in the opposite direction, for instance, in hoisting motions of cranes, where the load will be self-sustaining, with a single or double-threaded worm. Worm gear is somewhat expensive to make. Spur gearing is largely employed, and cut gears are used in nearly all cases for high-speed work. The use of raw-hide pinions has proved of great advantage in motor reduction gearing, and runs very quietly. Chain gearing is very suitable in many cases where the drive is short, though too great or not rigidly enough fixed to enable spur gearing to be used. It is generally less noisy than spur gearing, and has the advantage of positive drive, which belting has not. Direct-coupled motors are useful for machinery which runs at a high speed, such as fans, blowers, grinding machinery, &c. For slow-speed machinery they become very heavy, and, of course, more costly, and rather less efficient; though the absence of noise and wear and tear of gearing, &c., compensates for these points in many cases.

In conclusion, it is not necessary to dwell upon the advantages of electric power transmission in general, over shafting

and gearing in factories—these are becoming fully recognised in all branches of manufacture, and the reasons are not difficult to find. In nearly all cases works grow in size, and are extended and added to bit by bit. A new pattern shop, new foundry, or a shop for any special purpose may be built quite apart and any distance from the main building. It is hardly necessary to state how much easier it is to provide these detached shops with power by means of a couple of wires and a motor, than in the old method of erecting a long line of steam piping or line of shafting, involving perhaps the use of bevel gearing or half-twist belts, to say nothing of the difference in efficiency. If works were put down at the outset for the maximum output they were ever to attain, some of these advantages of electric driving would disappear; but is this ever the case in business? A works must grow, like everything else, in the natural course of things.

Everyone engaged in the trade—or shall we say, "profession?"—of electrical engineering is anxious to see the use of motors extended, and if this short paper prove of some slight assistance towards that desirable end, by directing attention to their application to this special class of work, the author will not have occupied your time in vain.

We are indebted to the courtesy of Messrs. J. H. Holmes and Co. for the series of lantern slides with which the paper has been illustrated. Many of these have been specially prepared during the last week for the purpose by their works photographer.

Mr. Moir.

Mr. ALEX. MOIR: I think Mr. Ralph's paper has been an eminently practical one, all the greater in value because it has dealt to a great extent with motors in actual use at Messrs. Holmes' works. Recently I had an opportunity of going over a new and fairly large printing works in Yorkshire, where electric driving had been adopted. Each printing-press was supplied with an independent motor, and the proprietors, who had carefully balanced the relative advantages and costs of steam and electricity, were more than satisfied with the result of their decision in favour of the latter. They had saved considerable capital outlay, and owing to its being possible to run just such motors as were really required to do actual work, at any hour of the day or night, without having to pay for attendance upon a steam engine plant, turning round shafting that would sometimes have been only very lightly loaded, real economy had been effected. On the other hand, an instance recently came under my notice which proved, as Mr. Ralph has indicated, that electro-motors cannot compete at present with other

descriptions of motive power, unless the economies that the use of motors renders possible can be practised. In this case a gas engine was used for driving a central shaft from which several light machines in a machine-tool shop were driven. The weekly gas bill was £1 15s. To have replaced this engine by a single motor—it was not possible to use more than one—would have cost, at existing Tyneside energy rates, just £4 per week. Mr. Moir.

Mr. C. TURNBULL: I think the tendency in the future will be more and more to use portable electrically-driven tools, especially for big work. By this means the weight of heavy castings will cease to be a cause of great expense in working, as they will not need much moving. Mr. Ralph mentioned a case in which a temporary lathe was rigged up for boring out a magnet, and it answered very well. The question arises, Why not always use such methods and save the cost of large tools? It would seem also that, in works where dynamos are made, generator-sets on test might be used to provide power for use in the factory. If this could be done, instead of all the power being sent to waste, there would be a great saving. The voltage could be adjusted by negative or positive boosters, while the load could be kept fairly steady by means of supplementary resistances. Integrating wattmeters could be used to measure the energy given off by the generator, so that there would not be trouble from unavoidable variations in the current due to machines driven from the generator stopping and starting. Mr. Turnbull

Mr. F. BROADBENT: I agree with the previous speakers as to the practical nature of this paper by my colleague, Mr. Ralph. It is all the more valuable in that it tells us what has actually been done and proved by experience to be good rather than theorises as to what might be done. It is a little disappointing, however, that no general principles are laid down as to the best methods of motor driving, the facts being in most cases merely stated and no conclusions drawn. Mr. Broadbent

In most of the examples given the question of convenience appears to have been the chief consideration rather than that of cost: that is to say, that it has generally been found more convenient, having cables handy and a scrap motor in stock, to make use of these instead of fitting up new shafting and pulleys to drive the particular machine under consideration. In many of the examples given the combined machine and motor have no doubt paid for themselves in a very short time, and this, I believe, applies more particularly to the boring mill mentioned, which paid for itself on its first job. We cannot, however, generalise from these data, and say that it will always pay to drive a boring mill by a motor. No; the special conditions obtaining in this case are responsible for the success. This bears out Mr. Ralph's remark that each case must be considered on its merits. Apart from special conditions, however, one would think that some general rule might be made for machine tool driving in ordinary engineering shops as to the smallest size of motor it is practicable and profitable to use. It is on this point I should like to have heard more.

As to the losses in shafting, we have come to believe that 50 per cent. is a fair average efficiency to obtain for shafting, and I was some-

Mr. Broadbent. what surprised on recently visiting a large engineering works in Bradford, where certainly the advantages of motors were understood, to find that motor driving was only adopted in a few cases, whilst the main shafts, newly put up, were driven by a steam engine. The owner assured me that from careful tests of the H.P. used in driving the shafting light, that is doing no work, but with all belts on, he found the efficiency to be 85 per cent. It would not have been possible by separately driving the machines by motors to have improved on that.

Notwithstanding this, however, the owner intended taking one or two of the largest machines off the shafting, and putting in separate motors, because of the time wasted in shifting the belts on the coned pulleys to get the various speeds required. Now, it is on this point that I must disagree with Mr. Ralph. On page 547 he says, "The average machine hand will not take the trouble to shift his belt to suit the size of the work unless he is constantly watched by the foreman." He puts this down to laziness. It is not altogether laziness. In the case just referred to, the machines were very large, and it would take two men a considerable time to shift the heavy belt from one pulley to the next, and as this speed might not be required for very long, the same amount of time would be wasted in shifting the belt back again in a short time. Now, if the man had kept the belt on the slow speed all the time, and never stopped, he would probably have turned out just as much work. It is here, then, that the advantage of motor driving comes in. Instead of wasting time shifting a heavy belt, or alternatively keeping the machine on the slowest speed, you have only to move the handle of the regulating switch up a few notches to get just the speed required. In this way the output from any variable-speed machine can be very considerably increased, as, instead of being tied to three speeds, as in cone-pulley driving, any intermediate speed can be got to suit the work. In the case of printing machine driving, the use of motors has in many cases increased the turnover by 25 per cent., and as wages and standing charges are not increased, this, of course, means greater profits, and the cost of the motor is soon paid for. It is sometimes difficult to show on paper any advantage in installing a motor. This applies more particularly to comparisons with gas-engine driving. Gas seems to be far cheaper on a steady load. In spite of this, cases are known in which a saving has been shown by replacing a gas engine by a motor. The reason is that, except in rare cases, the load on a shaft is not constant, and whereas a gas engine takes very little note of the variations in load, but just consumes as much gas as though doing its maximum output all the time, a motor does, on the contrary, note every little irregularity, and its consumption of energy varies in proportion.

These two points—the variation of load, and the variation of speed—are to my mind very important factors to consider in estimating the costs of alternative methods of driving.

Mr. Dobbie. Mr. R. S. Dobbie: I wish to join in congratulating the author on his paper. In speaking of the loss in shafting, I have seen several cases where good shops, properly laid out with hangers of the best description, have been changed over from a steam or gas-engine drive to an electrical drive. In one case a line of shafting 320 feet long,

driving eight or ten very heavy machines, absorbed something like 14 H.P. running light, with all the machine belts on loose pulleys. When all these heavy machines were running, the average power taken was at first about 28 H.P., and I thought we were overrating the power required for the machines, but found that it was one of the snares which the author has mentioned, and that, on occasion, much more than the 40 H.P. provided was called for. Certain machines appear to take but little power, but the conditions, if changed, show otherwise. In the case of the power necessary for a generator, I have lately installed a 32-k.w. generator for driving shafting in an emergency, due to a fire. Having had a little trouble, I found that we had a load as high as 70 k.w., and an average of 40 k.w. was called for, which shows how carefully we must install these things in order to get good results. It is not only the initial estimation that must be considered, but the increments that come from continued extension. The author mentioned a 10-H.P. motor demanding 60 H.P. I would like to ask how much came out of the motor under these circumstances. I can sympathise with the trouble of the lecturer with the electrically-driven single-motor cranes. I know of rope-driven ones that have been converted into single-motor cranes, but they have never worked satisfactorily. I have seen large castings held on a planing machine by magnetism. Machines of this type are used to a large extent in planing and grinding delicate work. In the case of large castings strips of iron and the like are used to prevent end movement. In America I once converted a shop, belt-driven from steam engine, into an electrically-driven shop; the factory consisted of five stories, including a basement, and was engaged in making small machinery weighing two or three hundredweight complete. The transmission was originally effected by means of belts and occasional bevel-wheels. These were all taken out, and a dynamo was run by the same engine that previously drove the belts, and the saving in power was something enormous. The factory originally took 260 H.P. to run everything, but when all the machinery was not in use it absorbed 160 H.P. In the case of electrical driving it took 160 H.P. to drive the whole shop, and when everything was turned off the H.P. indicated was only the friction load of engine and dynamos running light. Mr. Dobbie.

Mr. H. H. BIGLAND: Many of us have felt pleasure at hearing Mr. Ralph's paper, but there is one point that has been left rather vague. Several have spoken somewhat vaguely of small motors. I was hoping that the author would state the actual facts or would give his experience of the smallest-sized motors which he has put down. It is a matter that I have lately had under consideration. On referring one point to Messrs. W. H. Allen, Son & Co., Limited, the firm with whom I am, they have informed me that at their Bedford works, in twelve months, small portable drills have paid for themselves, and they are strong advocates of "small motors" (the term is vague, but we may at any rate say as low as ¼ H.P.) In most cases I have carefully looked into, I think they will be found economical. I have not, however, been able to see how, in the case of quite small lathes, the difficulty of the tool becoming jammed is to be sur- Mr. Bigland.

Mr. Bigland. mounted. I am anxious to know whether the fuses would blow, or what damage would accrue in direct driving. These are serious difficulties if occurring frequently. The question of gas-engine driving appears to be coming to the front if we credit the papers on Mona gas. Indeed, I have heard of a firm not very far from this town who have had experts engaged in sifting the matter thoroughly, and it appears possible that direct gas-engine driving coupling short lengths of shafts may be used by them in preference to electric motors. People seem rather to have ignored the fact that gas-engines now are worthy of consideration, and I think if any practical gas engineers are here they will question what Mr. Broadbent says about the consumption of gas being almost the same at full load as at half-load ; but these are all points that I would like information on.

Mr. Le Rossignol. Mr. LE ROSSIGNOL: I agree with the remark that one cannot so much discuss the paper as endorse all it enunciates. Mr. Ralph's remarks on the advantages of electrical driving are very much strengthened by the fact that this method of workshop driving is greatly used on the Continent and in the United States. I recently visited some of the largest locomotive and stationary engine works and large electrical works in the latter country, and endeavoured to find out why the Americans were able to turn out similar articles in quicker time than can be done in this country, and I can only attribute this to the far greater use they make of electrical driving. In these large works the principle had been largely adopted of tooling and finishing large castings by means of machines brought to the work. These machines were frequently of large size, were electrically driven, and could be lifted about and attached to the work without any difficulty, and thus large pieces could be completely finished without once being moved from their place or even turned over. Large generator-rings up to any diameter, fly-wheels up to forty feet in diameter, and other heavy pieces of machinery, were thus dealt with at a very small cost. In some of the large steel works the whole of the handling of the material was done by mechanical means : large electrical cranes lifted the ingots out of the reheating furnaces and deposited them on electrical travelling carriages. These carriages tipped them on live roller gangs, which carried them to the rolls, so that an ingot from its start from the furnace was not touched once by hand till it came to the cooling-table as a finished rail or girder.

There is no doubt that great economy in labour can be effected by an extensive adoption of electrical driving in all sorts of manufacturing processes, and in time I trust that manufacturers in this country will come to see the benefits which can be obtained by this means.

Mr. Heaviside. Mr. A. W. HEAVISIDE (*Chairman*): I do not know that I can add anything important to this very interesting subject ; but upon the question of the sub-division of power, I may say that on a visit to Switzerland I observed in a silk factory at Wädenschwyl, on the banks of the Lake of Zürich, that every girl operator had her own electric motor. These would be probably from $\frac{1}{8}$ to $\frac{1}{4}$ H.P., and we may be sure that, with a thrifty practical race like the Swiss, the motors would not be there if they were not commercially successful. In my opinion

Mr. Heaviside. there is no question that the electric motor must predominate if the current can be had at a reasonable rate, such as 1d. to 1½d. per unit, as is likely to be the case in this neighbourhood. In that case the gas-engine, with its cost of up-keep and the necessary special place it must occupy, is out of the running. You cannot put a gas-engine in a drawing-room, whereas you can put an electric motor there.

Mr. Ralph. Mr. G. RALPH (in reply): I must express my thanks to all the members who have taken part in the discussion for their kind remarks, and to the meeting in general for their kind reception of the paper.

Mr. Turnbull makes the suggestion that during the testing of dynamos, the power might be utilised for driving the machinery in the works, instead of running the current to waste in resistances. As a matter of fact, this *is* often done, when the voltage of the machines under test is at all suitable. It so happens that to-day the whole of the current required for power and lighting has been supplied to the works from a steam dynamo running under a ten hours' test. A considerable saving of fuel can be effected by this means. I am glad to hear Mr. Le Rossignol's confirmation of the views expressed as to the advantages of portable tools, &c. Mr. Moir's kind remarks do not call for any reply.

Mr. Broadbent thinks that too much stress is laid on convenience, rather than on relative cost. I admit that I have not attempted to go into the question of cost, with which he is far more competent to deal than I am. But, leaving capital outlay out of the question, it may fairly be said that when convenience is aimed at, and attained, cost (or wages) is reduced, and thus one factor is dependent on the other. He does not agree with me on the point of shifting of belts to get the correct speed on machines. Probably in the case of large heavy belts he is right, but with an ordinary 3″ or 4″ belt it is only a matter of a minute or two to shift from one cone step to another; and I think that most shop foremen and managers will agree with me that the neglect of utilising the means provided for getting the most work out of tools is largely due to laziness or indifference on the part of the operator. With regard to the losses in shafting, I think the owner of the particular factory referred to is to be highly congratulated if the loss is only 15 per cent.

Mr. Broadbent, and Mr. Bigland also, express disappointment because it is not stated definitely what is the smallest size of motor it is profitable and practicable to use for individual machine tool driving. I stated that I considered about 2 H.P. was a reasonable size to employ, but at the same time the conditions vary so much that one cannot make any very definite statement on this point.

Mr. Dobbie's remarks on shafting confirm what is stated in the paper. With regard to single motor cranes, it was only in one particular instance that I had an unhappy experience, and I am rather surprised to hear that he has found converted cranes unsatisfactory with a single motor drive. The one to which I referred was a new crane, and would have proved unsatisfactory with any method of driving, for, as explained, the bad workmanship was largely at fault. The case mentioned of a 10 H.P. motor demanding 60 E.H.P. is a very

Mr. Ralph. extreme case, and, of course, the C^2R losses would be very great, and the efficiency correspondingly low. I am much interested to hear of the magnetic hold-on devices for chucks and planer tables, and should much like to hear further details. I know that for grinding thin articles they are very satisfactory.

In further reply to Mr. Bigland, we have used motors of all sizes, from ¼ H.P. upwards. His remarks on portable electric drills fully bear out my own.

With regard to what happens to a small motor if the machine it is driving jams, or pulls up, probably the fuses would blow, or the belt would come off (and in practice, small machines usually are belt driven). In the case of a large machine driven by spur or chain gearing, if there is no overload release on the starting switch, probably the fuses will blow, which is not a very serious matter, although Mr. Bigland seems to think so. If it happens *frequently*, as he suggests, then there would probably be something the matter either with the method of doing the work, or with the workman, which would require prompt attention, not necessarily of an electrical nature! I must confess my surprise at a number of gas engines being used in any works to drive short sections of shafting in preference to electric motors. I do not imagine it is a method which will be largely adopted. I quite agree with Mr. Patterson's remarks on the grouping of small machines on to one line shaft.

Mr. Heaviside mentions an interesting example of individual driving, where the sub-division of power is much greater than is usual in this country; but there may be special circumstances in the particular case which justify it. The point he mentions about the space occupied by gas engines, compared with electric motors, is an important one.

GLASGOW LOCAL SECTION.

The Glasgow Local Section of the Institution of Electrical Engineers met in the large hall of the Institution of Engineers and Shipbuilders in Scotland, 207, Bath Street, Glasgow, on Wednesday, the 13th of February, Professor Magnus Maclean (Vice-Chairman) in the chair.

The CHAIRMAN explained that this was the first meeting since the Nation was thrown into mourning by the lamented death of the late Queen, and he stated that the Committee had held a special meeting and had, on behalf of the Section, passed resolutions of condolence and loyalty, which had been forwarded to the Home Secretary, and he asked the present meeting to confirm the resolutions so passed, which were: "That this Special Meeting of the Committee, on behalf of the Glasgow Local Section of the Institution of Electrical Engineers, desires to express its deep sense of the loss sustained by the death of their late beloved Sovereign, Queen Victoria, whose long beneficent reign has been marked by such social and scientific progress"; and "That an expression of confidence and loyalty be sent to His Most Gracious Majesty King Edward, with the hope that he might long be spared to reign over an attached people."

A METHOD OF COMPENSATING VOLTMETERS FOR THE VOLTAGE DROP IN LONG FEEDERS.

By MICHAEL B. FIELD, Member.

A problem which often confronts the central station engineer is the measurement at the power-house itself of the voltage obtaining at distant parts of his network which supplies current for, say, lighting and power purposes. In fact, it is very often of far greater importance that the engineer in charge should know accurately the value of the voltage existing at the distributing network, or, better, the lamp terminals or motor terminals, as the case may be, than that obtaining at the central station bus-bars themselves. In cases where the distributing network lies at some con-

siderable distance from the power-house, and is fed with current by means of "feeders," the drop of voltage in the latter may vary from zero, when the network is very lightly loaded, to 10 or 12 per cent. when the network is heavily loaded. Under such circumstances the engineer requires, of course, so to regulate his generators that the voltage at the network or lamp or motor terminals remains constant; in other words, he must raise the bus-bar voltage in the power-house above the normal by an amount equal to the drop in the feeders.

In such cases a very usual course to adopt is to mount on the switchboard in the central station voltmeters which are connected to points of the network by means of "pilot" wires, and thus indicate the true voltage existing at those points. The engineer in charge can then readily regulate his generators so that, under varying conditions of load, the voltage indicated by these voltmeters is maintained constant.

In the year 1882 Dr. Hopkinson patented an arrangement for effecting the same purpose without resorting to the employment of pilot wires. His method consisted of compounding the shunt windings of electromagnetic voltmeters with a few turns of thick wire, through which the main feeder current, or a proportionate part thereof, flowed. In this way a voltmeter connected to the near end of a pair of feeders might be compensated for the drop along the feeders, and would thus indicate the voltage existing at the far ends, and obviate the necessity of employing pilot wires.

There is, of course, no great disadvantage in employing pilot wires for the purpose named beyond the cost of the same, which will amount to at least £90 per mile per twin wire. If it be possible to save this sum, which, especially in the case of long-distance transmissions or networks covering extensive areas, will amount to a very considerable sum, by merely adding a suitable arrangement of resistances at the switchboards, it will of course be advantageous to do so. Moreover, by obviating the pilot wire, we obviate the trouble often connected therewith; and while speaking on this point I would call attention to the fact that it is by no means uncommon for pilot wires to give trouble. What the reason usually is I do not know. Whether it is that while laying them they are considered as an adjunct of no vital

importance, and therefore the necessary care is not devoted to their instalment, or whether it be for other reasons, is not a matter for discussion here.

Compounding coils of the above description are of course quite inapplicable to voltmeters constructed on the D'Arsonval principle, *e.g.*, Weston voltmeters, and in consequence of the almost universal preference shown by central station engineers for instruments of this class for continuous-current work, the writer some little while ago devised a method whereby any voltmeter whatever may be compensated for the feeder drop of either a two, three, or multiple wire system without necessitating constructional modifications of any kind, and, beyond that, can be arranged to indicate the *average* voltage obtaining at *any group* of distant feeding-points.

The method is capable of very general application, but three instances of its use are of special interest to central station engineers, and it is the object of this short paper, firstly, to describe briefly these three applications, and, secondly, to investigate analytically the accuracy of the same. The three cases for consideration are :—

(*a*) The measurement on the same instrument not only of the actual voltage existing at various feeding-points of a two-wire network, but also the *average* of the voltage at all the feeding-points, or the average voltage at any desired group of feeding-points of the network.

(*b*) The measurement of the individual feeding-point voltages and the average voltage at any group of feeding-points of a network on the three, five, or other multiple-wire systems.

(*c*) The measurement by means of a low-tension electrostatic multicellular voltmeter of the voltage at the far end of a long single- or multiphase-power transmission line working at extra high tension, and possibly possessing a comparatively large self-induction.

Let us imagine a large distributing network supplying current for lighting and power purposes over a considerable area. It is evident that a voltmeter in the power-house so

arranged that it indicated directly the average of the voltage all over the network would provide the engineer in charge with much more valuable information than one arranged only to indicate the actual voltage at some particular point of the network, for it is clear that the engineer should strive to maintain the average voltage at the normal value. Now, although it is not easy to obtain the true average of all points of the network, this may be approximated to by measuring the average voltage existing at all the feeding-points; this information would as a rule be a sufficient guide to enable the central station engineer to maintain the average voltage over the network itself at its normal value.

It might further be advantageous for the engineer to be able to measure the average voltage existing at some one group or other of feeding-points, *e.g.*, during one portion of the day he might perhaps wish to pay most attention to keeping the average voltage throughout the commercial portion of the town constant, and at another time it would be of more importance to maintain the average voltage throughout the residential portion of the town constant. He might further wish to know at any instant what was the actual voltage at the end of some one long feeder supplying an outlying district, more especially if this feeder contained a booster so that the voltage was capable of independent regulation; all of these conditions become possible by the method about to be described.

Case A.—The additional switchboard apparatus necessary for this case—for, say, a continuous-current system—consists of a low-reading Weston voltmeter with a suitably graduated scale (*e.g.*, an ordinary 600-volt Weston with the series resistance removed would serve perfectly), a multiple contact voltmeter switch, a set of voltmeter resistances which would be mounted in an out-of-the-way place at the back of the switchboard, and, further, a series resistance for insertion in each feeder circuit. These latter resistances would be similar to those usually supplied with Weston ammeters, and would be such that the drop across them with the maximum current would be of the order of $\frac{2}{10}$th volt, or less where exceptionally heavy feeder currents are dealt with.

The multiple contact switch would be arranged for as "positions" as groups of feeding points required.

Suppose, for example, three feeding circuits A, B, C left the station, a four-way voltmeter switch might be advantageously employed. With the switch successively in the first three

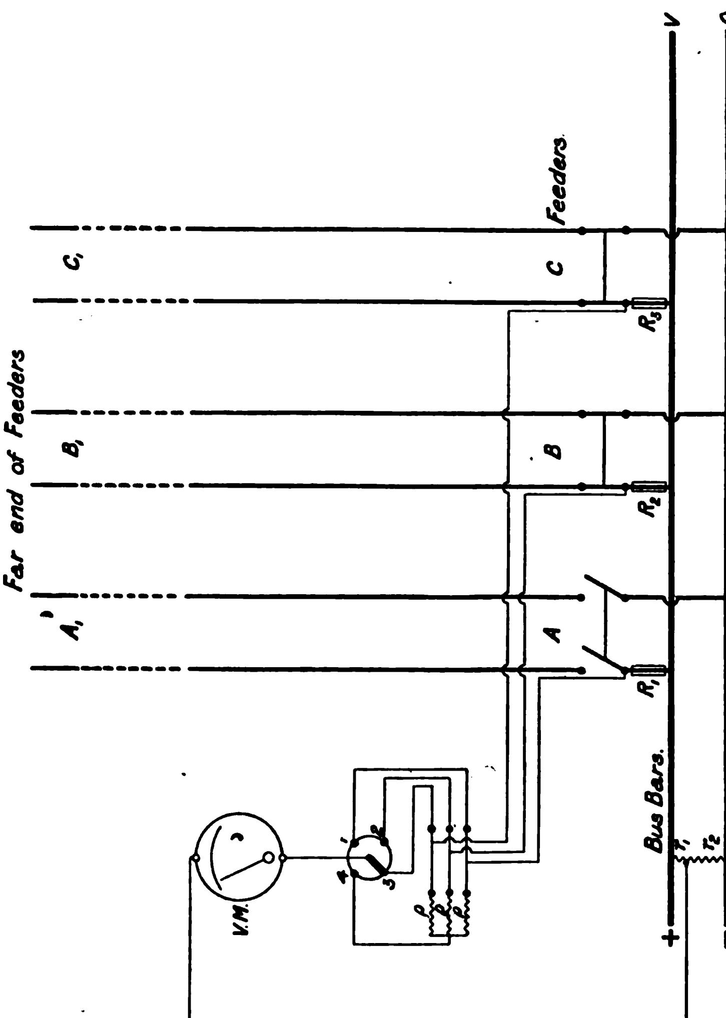

FIG I.

positions the voltmeter would indicate the feeding-point voltage of the network at A_1, B_1, C_1 respectively, while with the switch in the fourth position the voltmeter would indicate the average voltage at the feeding-points A_1, B_1, C_1.

Two distinct arrangements are shown in Figs 1 and 2.

Fig. 1 shows the simpler of the two, but involves the disadvantage that the average voltage of A_1, B_1, C_1 can only be obtained if all three feeding circuits are switched in. It is, moreover, only possible to determine the individual values A_1, B_1, and C_1 if the corresponding feeders be switched in.

In Fig. 2, on the other hand, a modification is shown whereby the individual voltages A_1, B_1, C_1, and the average of A_1, B_1, C_1, are given correctly by the voltmeter with the switch in the corresponding positions, whether some of the feeder circuits be switched in or not.

This involves extra complication in the multiple contact switch, in which case it may conveniently be constructed on the same lines as a miniature tramcar controller. In Fig. 2 the contact cylinder has, for the sake of clearness, been developed on a plane.

R_1, R_2, R_3 are the inserted series resistances in the feeder circuits; shunt resistances (divided into r_1 and r_2) are connected in Fig. 2 across each pair of feeders, in Fig. 1 across the bus-bars. The letter ρ represents that the particular connecting wires so distinguished must have sufficient resistance to prevent any appreciable interchange of current between the various feeder circuits when the switch is in the fourth position. V.M. is the voltmeter, and s a shunt which is connected across the terminals of the voltmeter when the switch is in the fourth position, in order to keep the calibration of the voltmeter correct.

Briefly explained the principle involved is as follows:—

If r_1 be $(\frac{1}{n})$th part of $r_1 + r_2$, the drop of pressure down r_1 will be $(\frac{1}{n})$th part of the bus-bar volts. Again, if R_1 be $(\frac{1}{n-1})$th part of the whole feeder (go and return) resistance, the drop along R_1 will be $(\frac{1}{n})$th of the total drop. The voltmeter then if connected to one feeder measures $(\frac{1}{n})$th part of the bus-bar voltage less $(\frac{1}{n})$th part of the drop, *i.e.*, $(\frac{1}{n})$th part of the voltage existing at the far end of the feeder. If a number of feeders be simultaneously con-

nected to the same voltmeter, this latter will indicate the average of all the individual voltages. The connecting wires must, however, as already stated, have sufficient resistance (ρ) to avoid any appreciable interchange of current between the different feeder circuits.

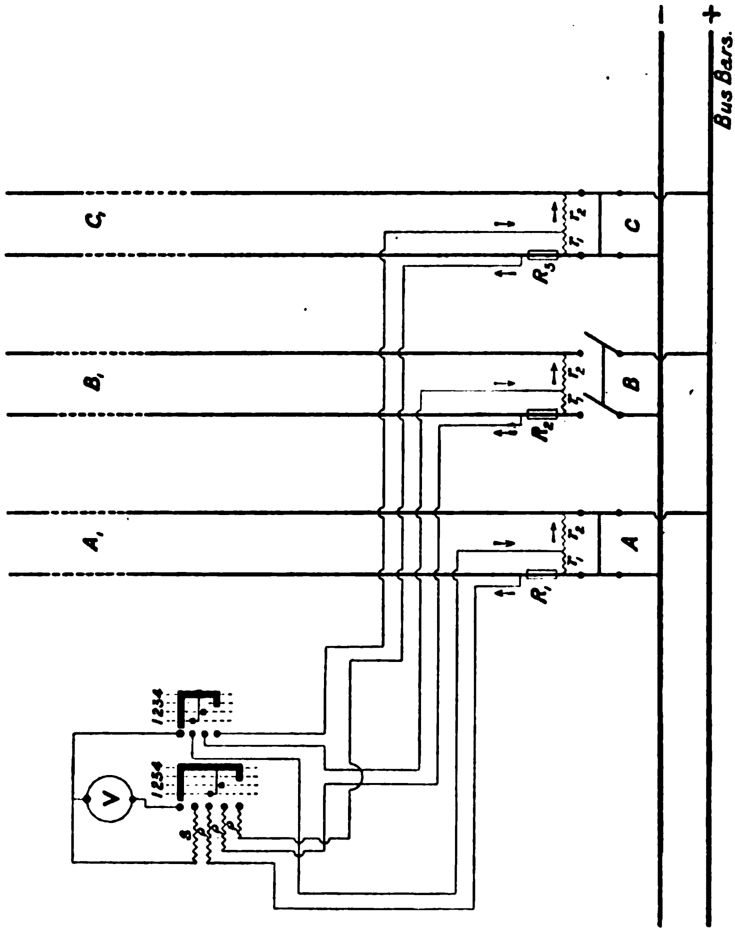

FIG. 2.

The action of the shunt *s*, which is necessary with the arrangement shown in Fig. 2, but not with that shown in Fig. 1, will be found fully explained in the analytical treatment of the subject.

It may here be mentioned that the writer, on subsequent investigation, found that particular cases of his method had

been anticipated by Mershon in America, and by Crompton and Ashley, and later by Heap, in England.[1]

Mershon's patents relate exclusively to alternating currents, since he bases them fundamentally on the use of either current or potential transformers, or both. Fig. 3 shows one of Mershon's arrangement, though many equivalent arrangements are illustrated in his patent specifications.

Mershon's object is to compensate the voltmeter not only for the ohmic but also for the inductive drop of the line, and his arrangement is only used in connection with single lines; *i.e.*, it corresponds to case (*a*), where the number of feeder circuits is unity.

As is evident from Fig. 3, the shunt resistance $r_1 + r_2$ of Fig. 1 is replaced by a potential transformer whose ratio

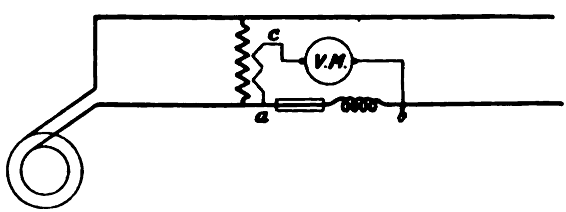

FIG. 3.

is $n : 1$. Further a resistance and a self-induction are inserted in the line whose values are $(\frac{1}{n-1})$th times those of the line resistance and self-induction respectively.

Referring to Fig. 3, we see then that the drop from *a* to *b* will be $(\frac{1}{n})$th of the whole drop and in phase with it, whereas the secondary voltage of the transformer is $(\frac{1}{n})$th of the generator voltage, and if the terminals be correctly chosen in phase with it. Thus the voltage *b c*, or that measured by the instrument, is $(\frac{1}{n})$th of that existing at the end of the line.

The anticipations of Crompton and Ashley, and later by

[1] Mershon : American Patent 551,982 ; dated December 24, 1895.
" " " 571,839 ; " November 24, 1896.
Crompton and Ashley ... 6,695 ; " ... ... 1898.
Heap ... ... 8,348 ; " ... ... 1898.

Heap, are merely case (*a*), where the number of feeder circuits is unity, *i.e.*, they compensate the voltmeter for the drop of voltage in a single pair of feeders by using shunt and series resistances as explained. The diagram of connections for this simple case is shown in Fig. 4, and needs no further comments.

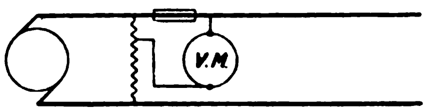

FIG. 4.

Neither Mershon, Crompton and Ashley, nor Heap, however, give any method for obtaining the average voltage at any desired group of feeding points, nor, with one exception referred to later on, is any method given in the above-mentioned patents applicable to case (*b*), *i.e.*, when unequal currents exist in the go and return feeders as in the case of three-wire systems and the like. This case, however, is just as important as the foregoing, owing to the extensive use now made of the three-wire system.

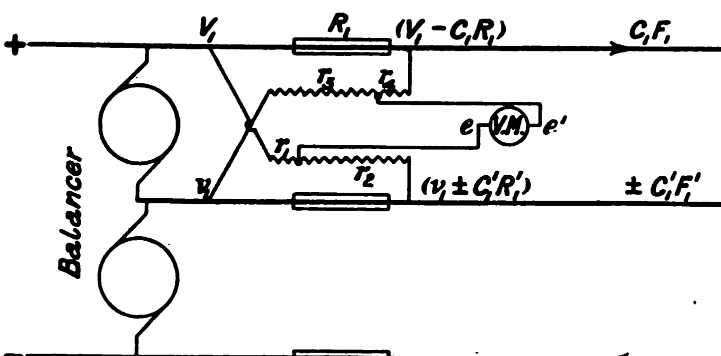

FIG. 5.

In such cases the voltage at the network between the neutral and each outer may be required to be known. The switchboard apparatus needed is very similar to that already described, the difference being that in this case series resistances must be inserted in *each* main, and the potential resistances are somewhat differently arranged

Fig. 5 shows the arrangement as applied to but one set of feeders. The voltmeter V will then be compensated for the combined drop both in the outer and neutral mains with which it is connected. If the average voltage between, say, the positive outer and the neutral at several feeding points be required, the arrangement of multiple contact switch as in Fig. 2 may be employed.

The principle involved in this case is somewhat different to that of case (*a*), and may be best understood by comparison with a Wheatstone's bridge.

Fig. 6 may be considered as equivalent to Fig. 5 if we

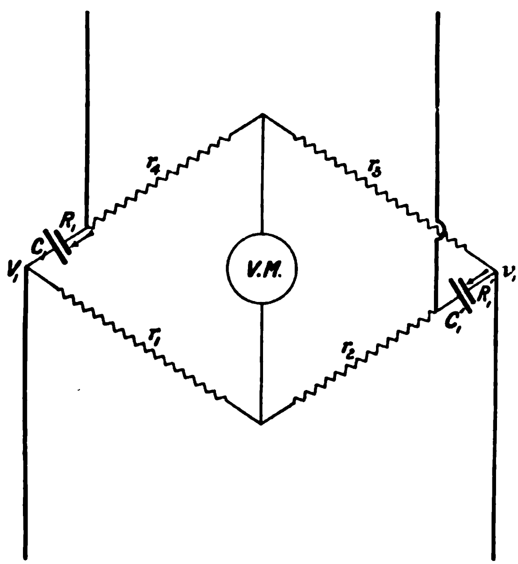

Fig. 6.

consider the potential differences called into play by the drop across R_1 and R'_1 as replaced by opposing E.M.Fs. If now the ratio arms of the Wheatstone's bridge be slightly out of balance, it is clear that the current through the voltmeter will be proportional to the expression $\{V_1 - v_1 - (\alpha\ C_1\ R_1 + \beta\ C'_1\ R'_1)\}$. Where α and β are numerics the value of which may be adjusted as desired by merely altering the resistance of the ratio arms. If, therefore, they be adjusted so that $\alpha\ R_1$ equals the resistance of the outer, and $\beta\ R'_1$ the resistance of the neutral, it is clear the voltmeter indications will be proportional to the voltage obtaining at the far end between outer and neutral.

With the device just described it is of utmost importance to select as sensitive a voltmeter as possible in order to minimise the losses which take place in the shunt resistances. In the latter portion of this communication this is shown to be the case, the following result being arrived at analytically :—

If the method be applied to a three-wire system with 400 volts between outers, and if the series resistances in the outer and neutral mains are as large as is at all practicable to make them (*e.g.*, the drop of volts along $R_1 = 0{\cdot}2$ volts and along $R'_1 = 0{\cdot}4$ volts with an average full load (the drop along the resistance R'_1 may be taken higher than along R_1 since the current in the former case is much less than in the latter, so that a larger drop does not necessarily imply a large loss of power), and if the voltmeter required $\frac{1}{100}$th ampere to give the full scale deflection, then the resistance of $(r_1 + r_2)$ and $(r_3 + r_4)$ could not well be more than 890 ohms. This would involve in them alone a continuous loss of nearly 90 watts. Instruments of the dead-beat suspended coil D'Arsonval type made up in switchboard form, with a needle swinging in a horizontal plane as in a Kelvin low-tension electrostatic voltmeter, would therefore be best for this purpose, for with suspended coil voltmeters of this description not only can the values of the resistances $(r_1 + r_2)$ and $(r_3 + r_4)$ be increased to 3,000 or 4,000 ohms, but the maximum drop of volts across R_1 and R'_1 may also be greatly reduced.

When employing this method for determining the average of several feeding-point voltages, a multiple contact switch as shown in Fig. 2 is necessary. It is also necessary to shunt the voltmeter by means of the resistance S, but the resistance ρ of the various connecting wires may be made as low as is convenient, since the resistances $r_1\ r_2\ r_3\ r_4$ will themselves prevent any appreciable interchange of current between the various feeder circuits.

Fig. 7 shows a particular case where the voltage at the end of a set of three-wire feeders may be simply arrived at. The figure is a sufficient explanation of itself. It is, however, clear that this case is rarely applicable in practice.

In Heap's Patent 8,348 of 1898, instruments of the Weston type are described for use in three-wire circuits, where the moving member has two distinct coils and four terminals

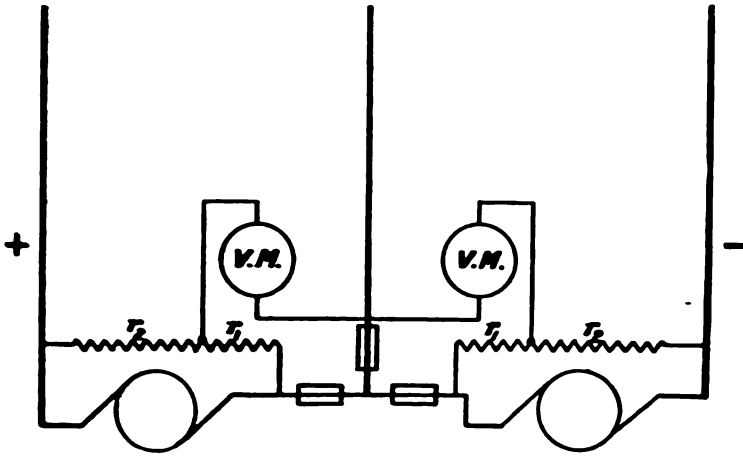

FIG. 7.

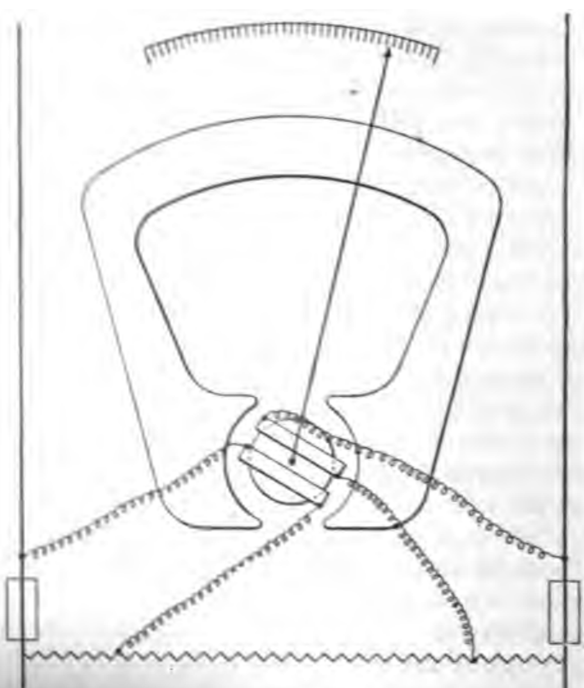

FIG. 8

as shown in Fig. 8. The great disadvantage of this arrangement is, of course, the difficulty of insulating the coils on the movable member from each other, the difference of potential being the full voltage between outer and neutral wire.

A further device is described, as shown in Fig. 9, with reference to instruments depending on the attraction of a small piece of iron. If these instruments be used at all, they may quite as readily be compounded with a few turns in series with the outer and neutral mains in accordance with the original scheme of Dr. Hopkinson. In any case,

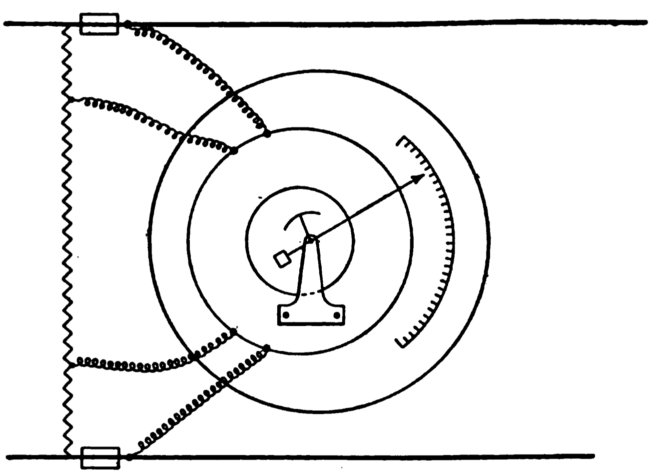

FIG. 9.

the device described above of Heap possesses the disadvantage that unless a large drop be allowed in the inserted resistances in the mains, the available voltage across the terminals of the voltmeter is altogether insufficient. For example, on a 250-volt circuit, if the series resistance caused an added drop of, say, 2 per cent. of the feeder drop, the available difference of potential for working the voltmeter would be only about 2·5 volts. With an instrument of the above type to be actuated by only 2·5 volts, a comparatively large current would be necessary, causing a relatively large watt-loss in the whole arrangement.

Case C.—This case, viz., the compensation for the ohmic and inductive drop in long alternate-current transmissions

lines has been very ingeniously handled by Mershon. As already intimated, Fig. 3 gives the pith of Mershon's arrangement, but in his American patents a considerable number of very pretty modifications are shown; but all of these, as he is careful to point out, depend upon the employment of potential or current transformers, or both.

The following method, therefore, which is quite independent of all transformers, not even necessitating one for the voltmeter, may be of interest.

In this instance not only must an extra resistance be inserted in the line, but similarly an extra self-induction, unless indeed a portion, say a $\frac{1}{300}$th part of resistance and self-induction of the line itself, be employed for the purpose by running back a pilot-wire to the station from a point on the line situated at a distance from the power-station equal to ($\frac{1}{300}$)th part of the whole length of the line.

To fix our ideas, let us consider a specific case where the line voltage is 30,000 volts, the line being 50 miles from end to end. At the station a 100-volt multicellular electrostatic voltmeter of the Kelvin type is employed, and connected as shown in Fig. 10. It would, of course, have to be thoroughly insulated from earth. k_1 and k_2 are condensers replacing the resistances r_1 and r_2 shown in Fig. 1. The condenser k_2 has a capacity approximately equal to the maximum capacity of the voltmeter, *i.e.*, the capacity between the fixed and moveable vanes, the latter being in the position they normally occupy when the voltmeter is indicating its maximum voltage. The capacity of the condenser k_2 is accordingly very small; it must, however, be capable of withstanding practically the full line voltage of 30,000 volts. The writer therefore proposes to employ for this condenser a slab of glass with pieces of tinfoil pasted on the two opposite surfaces, the slab being sufficiently thick to withstand the voltage, and the area of the tinfoil coatings being chosen to give a capacity approximately equal to the maximum capacity of the voltmeter. To prevent leakage and condensation on the edges of the slab, the whole might advantageously be immersed in a bath of special oil or be imbedded in a block of paraffin wax or other suitable dielectric.

The condenser k_1 must have a capacity nearly 300 times as great as that of k_2; since, however, the voltage across the

former will be of the order of 100 volts, it can readily be constructed in the usual way of sheets of tinfoil and mica, and will not be an expensive matter. It is important that the ratio of the capacities of $k_1 : k_2$ be maintained exactly, but it is not of any importance that the capacity of k_2 shall bear any very exact ratio to that of the voltmeter. Thus the voltmeter, if deranged, might be replaced by another of the same pattern, and the latter would indicate correctly. If a portion of the line itself be utilised, instead of inserting extra resistance and inductance a pilot-wire must be run back from a point one-third of a mile distant from the power house. This wire must be protected, of course, from the inductive action of the current in the line in a manner explained later on. One advantage in utilising a

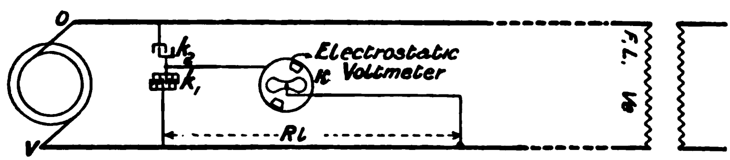

FIG. 10.

portion of the feeder itself instead of inserting extra resistances, etc, which may of course be done in this or any of the foregoing cases, is that no error is produced by a change of resistance of the line due to changes of atmospheric temperature.

We thus have a simple and inexpensive arrangement whereby a 100-volt electrostatic voltmeter may be arranged to measure directly the potential at the far end of a 30,000-volt line, the voltmeter being compensated for both ohmic and inductive effects of the line itself.

It is obvious that similar arrangements may be applied to many such useful purposes; *e.g.*, in the last case the voltmeter might be arranged to compensate not only for the line drop, but also for the drop due to both copper resistance and magnetic leakage in the transformer at the far end, and thus indicate the voltage at the low-tension terminals of the transformer irrespective of what kind of load may be connected to the same, *i.e.*, one possessing self-induction capacity or anything else.

[illegible]

[illegible] resistances inserted [illegible] each of these [illegible] be connected [illegible] resistance [illegible] the second terminal of the [illegible]. Calling the potential of this terminal [illegible], we may write—

$$[illegible] \quad C_2 R_2 \ldots C_n R_n) \quad \ldots \ldots \quad (2)$$

where $C_1, C_2, \ldots C_n$ are the various feeder currents, which may be taken as identical with the currents flowing through R_1, R_2, etc., provided ρ be sufficiently large to prevent any appreciable interchange of current between the various feeders.

We have further—

$$e' - e = i\,r \quad \ldots\ldots \quad (3)$$

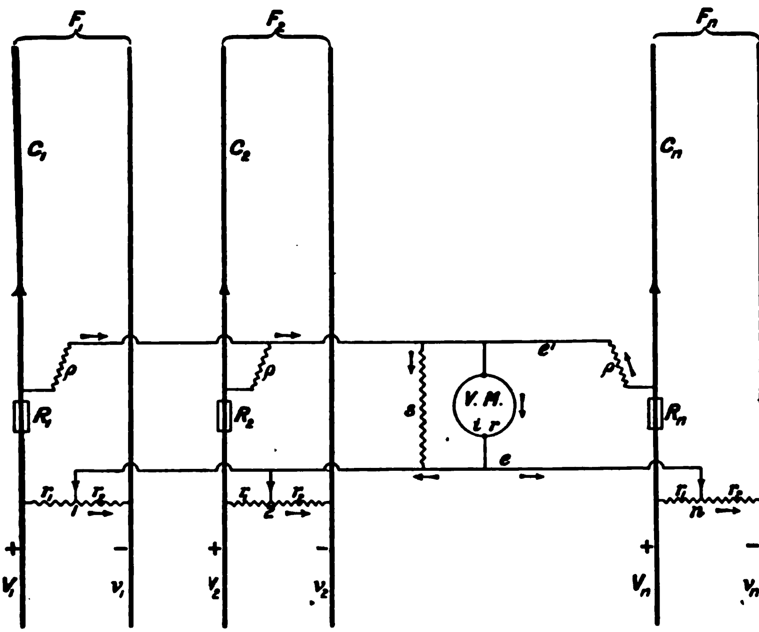

FIG. 11.

Hence, eliminating e and e' we have—

$$i = \frac{\frac{1}{n}\Sigma\left(V_\kappa - v_\kappa - \frac{r_1 + r_2}{r_1} \,.\, C_\kappa R_\kappa\right)}{\left(\rho \,.\, \frac{r_1 + r_2}{r_1} + r_2\right)\frac{s + r}{s\,n} + r\left(\frac{r_1 + r_2}{r_1}\right)} \quad \ldots \quad (4)$$

If

$$\frac{r_1 + r_2}{r_1} = \frac{R_1 + F_1}{R_1} = \frac{R_2 + F_2}{R_2} = \text{etc.},$$

the numerator of the above expression is merely the average

voltage existing at the far ends of all the feeder circuits. We will call this V_a. It is then clear that the current flowing through the voltmeter is proportional to V_a if any given number of feeder circuits be connected. The constant of the instrument, however, involves n; in order therefore to make the same calibration serve for all values of n, and to obtain the most sensitive arrangement, we must keep $\frac{s+r}{sn}$ equal to unity, in other words s must always equal $\frac{r}{n-1}$. This therefore is the function of the shunt s in Fig. 2, and if by means of the multiple contact switch, s be made to depend on n in the above manner, we have—

$$i = \frac{V_a}{r_2 + \frac{(\rho + r)(r_1 + r_2)}{r_1}}$$

If $n = 1$ we may make $\rho = o$ and $s = \infty$, *i.e.*, no shunt is required. This is the arrangement of Crompton and Ashley, and also of Heap.

The equation holding for the device represented in Fig. 1 is slightly different from the foregoing:—

We must insert in equation (1)

$$n = 1, \quad V = \frac{1}{n}\Sigma\left(V_1 + V_2 \ldots V_n\right), \quad o = \frac{1}{n}\Sigma\left(v_1 + v_2 + \ldots v_n\right)$$

and in equation (2) merely

$$V = \frac{1}{n}\Sigma\left(V_1 + V_2 \ldots V_n\right)$$

Combining (1) and (2), we have—

$$i = \frac{V_a}{\left(\frac{\rho}{n} \cdot \frac{r_1 + r_2}{r_1} + r_2\right)\frac{s+r}{s} + \frac{r(r_1 + r_2)}{r_1}}.$$

Now in this case, if ρ be very small compared with r, we may say that i does not depend on n, hence s may be made ∞, *i.e.*, no shunt is required, and we have—

$$i = \frac{V_a}{r_2 + \frac{r(r_1 + r_2)}{r_1}}.$$

Applying the above results to an actual case, let $V = 200$, and let the current required by the voltmeter for maximum scale deflection, corresponding to (say) 220 volts, be 0·01 ampere, the resistance of the voltmeter being 60 ohms.

Let, further—

$$\frac{F_1}{R_1} = \frac{F_2}{R_2} = \text{etc.} = 99,$$

then we have—

$$r_2 = 16{,}000 \quad \text{ohms.}$$
$$r_1 = 161{\cdot}6 \quad ,,$$

We may take it that the maximum feeder drop will not exceed 10 per cent., hence the maximum volt drop along R_1, R_2, etc., cannot be greater than 0·2 volt. If, therefore, $\rho = 0{\cdot}75$ ohm, we may say that the interchange of current between any two feeders cannot be more than 0·13 ampere, or an amount which is quite inappreciable, and at the same time the maximum error introduced by the variation of the value of $\frac{\rho}{n}$ as n is varied cannot exceed $\frac{1}{5}$ of one per cent.

With the arrangement shown in Fig. 2, ρ must be much greater than heretofore assumed, for if one set of feeders be switched out, the voltage at the near end of the positive of the same might differ by as much as 20 volts from that of neighbouring positive feeders, and a serious interchange of current between them would occur if ρ were not of considerable magnitude. Since, however, from equation 4 it appears that no error whatever is produced by the variation of n, provided s is always equal to $\frac{r}{n-1}$, we may, if we like, take $\rho = 100$ ohms. In this case, taking the same constants for the voltmeter as before, r_2 will come out 6,000 ohms and r_1 60·6 ohms.

It has been assumed throughout the foregoing that the same current returns by the negative feeder as goes by the positive feeder of the same circuit; this will generally be true, unless indeed the resistances of the positive and negative feeders are different, or the problem is complicated by the fact that there are faults at different parts of the system. In such cases, however, where the outgoing current is of

different magnitude from the return current, the following arrangement might be employed, which moreover is particularly applicable to three-wire systems and such like.

To derive an expression for i in case (b)—see Fig. 5—we have from equation (1)—

$$e = \frac{r_2}{r_1 + r_2} \cdot \frac{\Sigma V_\kappa}{n} + \frac{r_1}{r_1 + r_2} \cdot \frac{\Sigma v_\kappa \pm C'_\kappa R'_\kappa}{n} + \frac{r_1 r_2}{r_1 + r_2} \cdot \frac{s + r}{s} \cdot \frac{i}{n} \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

To obtain e' we must substitute in this same equation—

e' for e		$V_\kappa - C_\kappa R_\kappa$ for V_κ
r_4 „ r_1	v_κ	„ $v_\kappa \pm C'_\kappa R'_\kappa$
r_3 „ r_2	$-i$	„ i

and we may further write $r_1 + r_2 = r_3 + r_4$,

hence—

$$e' = \frac{r_3}{r_1 + r_2} \cdot \frac{\Sigma V_\kappa - C_\kappa R_\kappa}{n} + \frac{r_4}{r_1 + r_2} \cdot \frac{\Sigma v_\kappa}{n} - \frac{r_3 r_4}{r_1 + r_2} \cdot \frac{s + r}{s} \cdot \frac{i}{n} \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Combining (3), (5), and (6) we get—

$$i = \frac{\frac{i}{n} \Sigma \left(V_\kappa - v_\kappa - C_\kappa (R_\kappa + F_\kappa) \mp C'_\kappa (R'_\kappa + F'_\kappa \right)}{(p r_4 + q r_2) \frac{s + r}{s n} + \frac{r (r_1 + r_2)}{r_3 - r_2}} \quad . \quad (7)$$

where

$$\frac{r_3}{r_3 - r_2} = 1 + \frac{F_1}{R_1} = 1 + \frac{F_2}{R_2} = \text{etc.} = p,$$

and

$$\frac{r_1}{r_3 - r_2} = 1 + \frac{F_1'}{R_1'} + 1 \frac{F_2}{R_2'} = \text{etc.} = q$$

Now the numerator in (7) is the average voltage obtaining at all the far ends of the feeder circuits between the outers F_1, F_2, etc., and the neutrals F_1', F_2', etc. Call this average voltage, as before, V_a.

If, then, we again, by means of the multiple contact switch, keep $\frac{s + r}{s n}$ equal to unity, we have for case (b)—

$$i = \frac{V_a}{p\,r_4 + q\,r_2 + \dfrac{r(r_1 + r_2)}{r_3 - r_2}},$$

from which the necessary values for r_1, r_2, r_3, and r_4 are calculable for any given instance.

The last case is that of (*c*), Fig. 10. We can readily derive the equation for the voltage existing across the voltmeter (which may be designated v') from equation (4), by writing :—

$n = 1$ $s = \infty$	$R + l\theta$ for R_1	$\dfrac{1}{k_2\theta}$ for r_2
$\rho = 0$ $v = 0$	$\dfrac{1}{k_1\theta}$ " r_1	$\dfrac{1}{\kappa\theta}$ " r;

where R and l are the resistance and self-induction respectively of that portion of the line spanned by the pilot-wire, κ is the capacity of the voltmeter, this being a function of the position of the movable vanes, and θ represents the operator $\dfrac{d}{dt}$.

We have then—

$$v' = \frac{i}{\kappa\theta} = \frac{V - \dfrac{k_1 + k_2}{k_2}\cdot\left(R + l\theta\right)C}{\dfrac{\kappa + k_1 + k_2}{k_2}}.$$

If—

$$\frac{k_1 + k_2}{k_2} = \frac{F}{R} = \frac{L}{l},$$

the numerator of the above expression represents the voltage obtaining at the end of the line. Call this V_e.

As already explained, k_2 may advantageously be made about equal to the maximum value of κ, in which case, owing to the comparatively large value of k_1, we may write with great accuracy—

$$v' = \frac{k_2}{k_1 + k_2}\cdot V_e.$$

It has been pointed out in the previous part of this paper that it is necessary to shield the pilot-wire from the inductive action of the line.

If the line be a single-phase one carried overhead, it will be simple to place the pilot-wire symmetrically with regard to the two-line wires, in which case all inductive effects will be obviated.

If the line be an overhead three-phase one, it is not always possible to place the pilot-wire in a symmetrical position with regard to all three-line wires. The inductive action may, however, be neutralised by running the pilot-

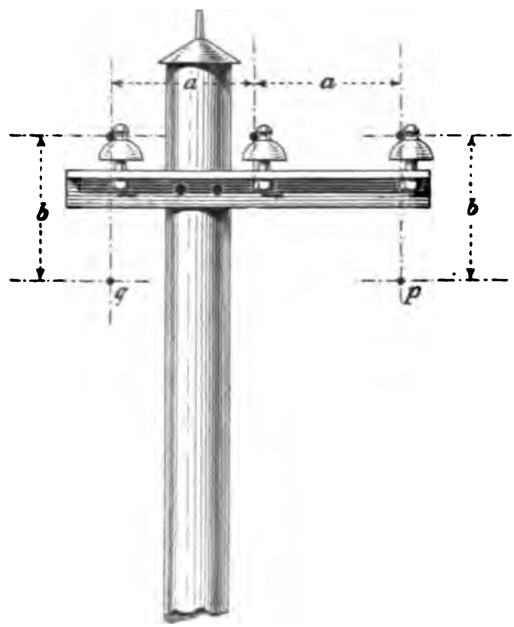

FIG. 12.

wire for *part* of the distance nearer the one or other of the lines. Thus, if the three-line wires be carried in a row (see Fig. 12) and a pilot-wire be run for a total distance of 500 yards from the station, it might be located for 250 yards in position *p*, and for the remaining 250 yards in position *q*. The inductive action of each line wire on the pilot-wire depends on the logarithm of the reciprocal of the distance between them plus a constant, which latter is the same for all the line wires.

If, therefore, we adjust matters so that

$$\frac{1}{2} \log. \left(\frac{1}{b}\right) + \frac{1}{2} \log. \left(\frac{1}{\sqrt{4a^2 + b^2}}\right) = \log. \left(\frac{1}{\sqrt{a^2 + b_2}}\right),$$

or

$$b = \frac{1}{\sqrt{2}} a,$$

the inductive action of each line wire on the pilot-wire will be the same, and since the total current flowing in the three-line wires at every instant is zero, the total inductive effect will likewise be zero.

With other arrangements of the line wires, corresponding modifications of the positions of the pilot-wire will be requisite.

APPENDIX.

(*Received February 23rd, 1901.*)

It is interesting to note that if the three lines of a three-phase transmission be arranged as shown in Fig. 12, the induced E.M.F. in each line due to the self-induction of its own current and the mutual induction of the currents in the other wires is not the same in each case, and consequently, in compensating for the line drop, this should be taken into account.

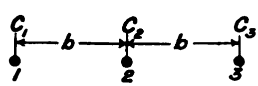

FIG. 1.

This inequality may readily be shown to exist thus :—Neglecting the magnetic induction within the line wires themselves, the total induction in air between two line wires at distance apart b and of radius a, per unit length of line

$$= \frac{4C}{10} \log \frac{b}{a}$$

where C = current in ampere in line. We may take therefore $\frac{2C}{10} \log \frac{b}{a}$ as the total induction cut by each wire, this being the sum of its own self-induced lines, and of those induced by its neighbour which it has cut. For the sake of brevity, this will here be called the inductive effect on the one-line wire.

Since the sum of the currents in the outers equals at every instant the current in the middle wire of the three-phase line, the inductive effect on the middle wire will be $\frac{2C_2}{10} \log \frac{b}{a}$ where C_2 is the instantaneous value of the current, it being quite immaterial whether all the return current be concentrated at one point or not, provided it all be at distance b from the line in question. (The inductive effect as defined above on *2* would be unaltered even though the return conductor were spread out as a thin sheath concentric with *2* and of radius b.)

The inductive effect on lines 1 and 3 respectively is per unit length :—

$$\frac{2}{10}\left\{C_1 \log \frac{1}{a} + C_2 \log \frac{1}{b} + C_3 \log \frac{1}{2b}\right\}$$

$$\frac{2}{10}\left\{C_1 \log \frac{1}{2b} + C_2 \log \frac{1}{b} + C_3 \log \frac{1}{a}\right\}$$

But $\left(\frac{2}{10} \log a\right)\left(C_1 + C_2 + C_3\right) = 0$, adding this in each case we have

$$\frac{2}{10}\left\{C_2 \log \frac{a}{b} + C_3 \log \frac{a}{b} - C_3 \log 2\right\}$$

$$\frac{2}{10}\left\{C_2 \log \frac{a}{b} + C_1 \log \frac{a}{b} - C_1 \log 2\right\}$$

These we may write as :—

$$\frac{2}{10}\left\{C_1 \log \frac{b}{a} - C_3 \log 2\right\}$$

$$\frac{2}{10}\left\{C_3 \log \frac{b}{a} - C_1 \log 2\right\}$$

and the inductive effect on line 2 is as above

$$\frac{2}{10}\left\{C_2 \log \frac{b}{a}\right\}$$

In an actual case let $\frac{b}{a} = 100$, and remembering that the induced E.M.F.'s lag 90° behind the currents which induce them, we may construct the vector diagrams for the induced E.M.F.'s in the three-line wires as follows :

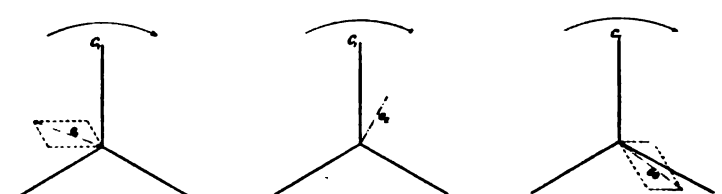

FIG. 2.

We see, then, that in the case of line 1, an E.M.F. is injected into the line 30° behind the current; in line 3 an equal E.M.F. appears 150° behind the current, that is, to say, that 3 and 1 are acting as the primary and secondary of a transformer power disappearing from the one and appearing in the other line. The amount of this injected E.M.F. is 15 per cent of c_3, that is in the above case, the relative values of the induced E.M.F.'s would be as 2·16 : 2 : 2·16 respectively for the lines 1, 2 and 3. In extra high tension transmission lines it is quite possible for the inductive effect to exceed the ohmic drop, so that the above differences may be worth considering.

The voltmeter in such a case would be arranged to measure the voltage between one pole and the neutral point compensating for the mean drop along the three-line wires, and the scale would be calibrated to [illegible] voltage.

The neutral point would be obtained by means of two suitable choking coils in the usual way, connecting one to the centre point of the winding of the other, as in the diagram.

It is further interesting to inquire into the magnitude of the inductive effect of a line upon a pilot wire which is not shielded.

As an example, assume 3 to be a pilot wire and 1 and 2 the go and return conductors of the circuit.

The inductive effect on 3 will be $\frac{2}{10}$ C log 2 and on 1 and 2 $\frac{2}{10}$ C log $\frac{b}{a}$ if C = line current. If $\frac{b}{a} = 100$ as before, the induced E.M.F. in the pilot wire will be 15 per cent. of the induced E.M.F. in each line wire. As stated above, this latter is sometimes greater than the ohmic drop, so that the effect on the pilot wire is seen to be of considerable magnitude; this is more especially the case where the pilot wire is worked at a reduced pressure by means of step-down transformers at the far end of the line, which is often the case owing to the liability of thin aerial lines to break, and the danger which would thus be introduced.

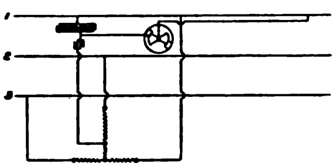

FIG. 3.

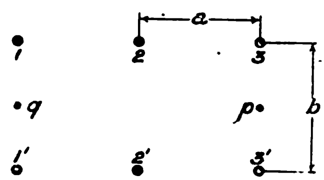

FIG. 4.

It is not always possible to shield the pilot wire from the inductive action of a multiplicity of line wires. In such a case it would be advantageous to change over the positions of the various wires periodically, so as to neutralise their effects.

In the case of two three-phrase circuits carried as in the fig., the pilot wire should be taken for half its distance in position p and the remaining distance in position q where $b = a\sqrt{2}$.

The Chairman.

The CHAIRMAN, in opening the discussion, remarked that the paper was not only a learned one from an analytical point of view, but also an exceedingly important paper from a practical point of view. Later, in the discussion he asked Mr. Field—(1) to give some relative statements as to the amount of energy wasted in the series resistances R_1, R_2, R_3, etc., and (2) to make it clear if these resistances were not exactly $\frac{1}{n}$th of the feeder resistances, whether or not the voltmeter needed recalibration in the central station.

Professor Jamieson.

Professor A. JAMIESON: Mr. Field has been rather lax in the phraseology of the first part of his paper, but he has made up for this defect by giving us a series of well-arranged and indexed diagrams, together with a final clear analytical investigation.

The chief object which the author had in view was to ascertain the drop in pressure along live feeders without using pilot-wires, and thu

Professor Jamieson.

not only to save their first cost and upkeep, but also to avoid being troubled by their possible defective insulation or continuity. He puts down the first cost of twin pilot-wires at £90 per mile. I think that this may be taken as a maximum value, since the trenches must be opened for the feeder-troughs or pipes whether pilot-wires are put down or omitted. Perhaps Mr. Field will state in his reply how he arrived at this price.

The average potential at the ends of any group of feeders might be obtained from the switchboard ends of the corresponding pilot-wires quite as easily as by the methods explained in the paper. There is, however, one disadvantage which Mr. Field's system has, when compared with the use of pilot-wires, viz., their employment (if sound) in localising faults in the feeders by means of "loop tests." And, moreover, he introduces a constant loss of power by the permanent insertion of the series-resistances R_1, R_2, R_3, etc. (see Figs, 1, 2, etc.), unless he can employ them as the feeder "fuse-wires," or for the lightning protector coils, or for the automatic switch coils. For example, suppose the current conveyed by each of these several resistances to be 1,000 amperes with a difference of potential between their ends of only 0·2 volt (as assumed in the paper), then the continuous loss of power will be $(1{,}000 \times 0{\cdot}2) =$ 200 watts for each inserted resistance. This quantity, when added to the losses $(r_1 + r_2)$ may thus amount to about $\frac{1}{4}$ H.P. at the switchboard or, say, $\frac{1}{2}$ H.P. in the boiler. The annual value of this loss would go far to pay interest and depreciation upon ordinary pilot-wires.

Mr. Field's ingenious methods of obtaining the average voltage at the ends of a group of feeders may no doubt satisfy the Board of Trade authorities, who demand statements of such averages in the station log-sheets, but great care must be taken that switchboard attendants do not place too much reliance upon such averages. One consumer or district may be supplied at 10 per cent. more than the normal voltage, whilst another is receiving 10 per cent. less than the normal. The attendant when reading the voltmeter in contact with the average terminal would consequently find that the mean voltage coincided with the required pressure. This would no doubt please him, and if he were lazy and not very particular, he would not trouble to ascertain what the respective voltages were at the end of each feeder.

The method (see Fig. 1) of ascertaining the voltage at the far end of a line of known resistance, by taking the potential difference between the ends of another known resistance placed in circuit with the former at its home end, is not new. I have frequently used this test since 1872, and I believe that it was first devised by Mr. Latimer Clark and improved by Professor Fleeming Jenkin.[1] Fig. 2, for obtaining averages, is, however, new to me, and I have much pleasure in congratulating Mr. Field upon the devices and their explanations as illustrated by Figs. 4, 5, 6, and 10. The latter is especially neat and interesting, although the substitution of condensers for resistances is by no means new. Their use and interchangeability in the case of the bridge-arms of artificial lines for working submarine cables upon

[1] See Munro and Jamieson's *Pocket-Book of Electrical Rules and Tables.*

Professor Jamieson.

the duplex principle has been employed for nearly a quarter of a century. Their application, however, to very high alternate current voltages is one of the difficult experiments upon which we should have been glad to have had some practical results. Perhaps Mr. Field may soon be able to bring before us data of actual tests and thus add to the value of his present paper.

Mr. McWhirter.

Mr. WILLIAM McWHIRTER: Mr. Field has written a splendid paper and has shown how to carry out an important test in a very elegant, simple, and economical manner. Never having been a Central Station engineer, I have not been in a position to discover all the beauties which these engineers have found in the Weston Moving Coil Voltmeter. No doubt, owing to the great sensibility of this type of instrument and to the low power therefore required to operate it, together with the proportionality of the scale divisions and its *supposed* extreme constancy, this instrument has made many friends. To these qualities must be added its freedom from external magnetic fields, from which it is usually *supposed* to have perfect immunity. The simpler and cheaper, yet equally useful, electro-magnetic voltmeter has therefore been relegated to a position of obscurity, and the individual who attempts to check the voltage of any installation by such means runs a risk of being driven from the society of all self-respecting electricians. Yet there are hundreds of thousands of those unassuming instruments in daily use doing good work. Those who have been able to watch the evolution of this type of instrument since 1878, when the electric lighting at the Paris Exhibition showed the need for it, must have often asked, "Have we yet approached finality?" Almost the first attempt to supply the demand for such measuring instruments was met by Deprez, who introduced an instrument of the permanent-magnet type. We had next the very excellent but certainly awkward set of dynamometers by Siemens Brothers, where the value of current or pressure could be easily ascertained by twisting the torsion-head until the moving coil reached zero. When, reading the angle of torsion in degrees, and finding its square root and multiplying by a five-figure constant, the amperes or volts were at once found. So beautiful and simple an instrument was not, however, received as it should have been, and I have known engineers in charge of lighting in those early days, and drawing the splendid salaries of from 18s. to 22s. per week, object to the use of the instruments.

It always seemed strange to me that only one man up to that time had made instruments to read direct in amperes, or, rather, at the time to which I refer, in webers and volts, and that was Mr. J. T. Sprague, of Birmingham. I would like, however, to warn central-station engineers not to take too much for granted about Weston moving-coil meters, not to be sure the scale divisions are strictly proportional, nor to assume the *constancy* of magnetic field, or to take it for granted that these meters are *not* affected by external magnetic fields. I have proved over and over again that every one of these assumptions is in error. Now that shielded electro-magnetic volts and ammeters are obtainable where less than two watts are used upon a circuit of 100 volts, where the tempera-

Mr McWhirter. ture coefficient is *nil*, where the scale error is under 1 per cent. and absolutely constant, and where magnetic-field errors do not exist, they are worthy of further attention, and should not be lightly abandoned. Further, such instruments can be calibrated to read correctly upon either continuous- or alternating-circuits as required, and are therefore most suitable for use in the arrangement of Hopkinson, which, from its simplicity, will be very hard to supplant.

Mr. Field. Mr. M. B. FIELD in reply said: Professor Jamieson commented at considerable length on the phraseology used by me in the opening sentences of the paper. I have no doubt that this might be much improved, but provided the paper represents correctly the technical points I wished to bring before you, I think it hardly worth while to waste further time in discussing the point. Professor Jamieson seems to think that measuring the "average voltage" over a network is useless. I admit it does not give all the information required, but then, by the methods I have described to-night, not only the average, but also the individual feeding-point voltages can be determined. In such a case I think that a little consideration will show that the information obtained *is* of distinct advantage. The price of £90 per mile of twin pilot-wire has been thought excessive. This, however, was not a guess-work figure: it was the actual figure for a two-core paper-insulated, lead-covered cable capable of withstanding 1,000 volts between cores, or between cores and lead, and includes drawing in and jointing. It is by no means a high price. Professor Jamieson tells us that the method I have described for compensating voltmeters as due to Crompton and Ashley did not originate with them. I believe, however, they took out the original patents. Answering the remarks on the applicability of the methods described to power and traction schemes, I would say that an exact regulation of the feeding-point voltage on tramway systems is not at all essential. The reference made in the paper to power systems applies to such as include the driving of motors of cotton-spinning machinery and such like where an exact and constant speed is of the utmost importance. Professor Jamieson further remarked that the different temperature coefficients of the series resistances R, R_1, etc., and of the line would produce errors. It is evident, however, that since we are compensating the voltmeter for a quantity of, say, not more than 10 per cent. of the total measurement, a small percentage of this compensation becomes negligible. If we want to be very exact we can dispense with R, R_1, etc., altogether by using a portion of the cable itself and running a pilot-wire back, say, from the first junction-box along the route. Regarding the losses that occur in R_1, R_2, R_3, etc., I mentioned in the paper that for heavy currents the drop along these resistances might be reduced to, say, 0·1 volt at full load. This is about the order of loss in the ordinary ammeter shunts used for switchboards, and consequently the feeder ammeter shunts might quite well be employed for the compensating voltmeters (adjusting r_1 and r_2 suitably), so that no added loss by the inserted resistances would then occur.

I do not understand the difficulty of making a condenser to withstand a pressure of 30,000 volts in the manner I have described. There

is certainly no difficulty in making porcelain insulators to withstand this voltage, and if a correspondingly thick slab be used for the condenser no difficulty should be experienced. Mr. Field.

Mr. McWhirter spoke about my preference for Weston instruments. I think I pointed out that the methods dealt with in the paper are applicable using any type of sensitive instruments without constructional modifications of any kind. All that is necessary is a readjustment of resistances.

Replying to Professor Maclean, I would say that if R, R_n, etc., are not exactly right, no very great error is introduced. The voltmeter readings will merely compensate for the drop in a line a little longer or a little shorter than the actual one. It is not at all necessary to calibrate the instruments *in situ*. A spurious line circuit having the correct feeder-resistance is made up in the laboratory and connected to the instrument, which is easily calibrated under these conditions and the series resistances perfectly adjusted. The series resistances are precisely similar to those supplied with Weston ammeters, and therefore do not involve any greater energy losses than do these ammeters which are in very common use.

ORIGINAL COMMUNICATION.

NOTE ON RESONANCE WITH ALTERNATING CURRENTS.

By ALEXANDER RUSSELL, M.A., Member.

Although the effects of resonance in alternating current circuits have been well known for many years, it is only recently that it has been proposed to utilise these effects for testing and other purposes. Rosa and Smith[1] have utilised the rise of pressure due to resonance to measure more accurately the losses in a condenser. Lodge[2] makes use of it in his system of space telegraphy. Duddell has recently utilised a resonant circuit to get high frequency alternating

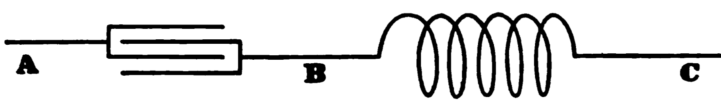

FIG. 1.

currents from the direct current arc between hard carbons. It has often been proposed also to utilise the resonance of currents in divided circuits, so as to raise the power-factor of the mains. It is therefore of importance that electricians should have clear ideas on this subject.

The sine curve theory is fairly satisfactory when only used as a rough guide to what happens in practice, but there are many important cases where phenomena happen which it completely fails to explain. In what follows I have attempted to fill up this gap in the theory mainly by considering numerical examples to show what we might expect when the wave is not shaped like a sine curve.

Consider an inductive coil (Fig. 1) in series with a condenser, and suppose that an alternating P.D. is applied to the terminals A and C. If the wave of P.D. be sine shaped, then it is easy to find the P.D.'s between A and B and be-

[1] *Phys. Rev.* 8, pp. 1–20, 1899; *Science Abstracts*, vol. ii., p. 243.

[2] *Jour*[illegible]*tution of Electrical Engineers*, vol. xxvii., p. 799.

tween B and C. Let the coil have a resistance R (ohms) and an inductance L (henries) and let the capacity of the condenser be K (farads). Now, by a well-known rule, we can replace the condenser by a coil whose resistance is zero and self-inductance

$$\frac{1}{K(2\pi f)^2}$$

where f is the frequency of the alternating current. Hence if C be the current and V, V_1 and V_2 the P.D.'s between A and C, A and B, and B and C respectively, then by the ordinary impedance formula,

$$C^2 = \frac{V^2}{R^2 + (2\pi f)^2\left\{L - \frac{1}{K(2\pi f)^2}\right\}^2}$$

$$= \frac{V_1^2}{\frac{1}{(2\pi f)^2 K^2}}$$

$$= \frac{V_2^2}{R^2 + (2\pi f)^2 L^2}$$

Hence V_1 and V_2 are found in terms of V.

Resonance occurs when

$$L K (2\pi f)^2 = 1$$

In this case

$$V_1 = \frac{2\pi f L}{R} V$$

$$= \frac{1}{2\pi f K R} V$$

$$V_2 = \frac{\{R^2 + (2\pi f)^2 L^2\}^{\frac{1}{2}}}{R} V$$

In this case V equals C R and the power factor is unity.

When the applied P.D. wave is not sine shaped the formulæ become very unwieldy. Instead, however, of solving this problem, we have considered the much simpler case of how the pressures across the condenser and choking coil terminals vary with the shape of the current wave. We are thus enabled to utilise some of the results given in the *Journal*, vol. xxix., p. 154.

Resonance of E.M.F's.

Let e, e_1 and e_2 be the instantaneous values of the volts between A and C, A and B, and B and C respectively, and let i be the instantaneous value of the current, then—

$$e = \mathrm{R}\,i + e_1 + e_2 \quad \dots\dots\dots \quad (1)$$

$$e_1 = \frac{\int i\,dt}{\mathrm{K}} \quad \dots\dots\dots \quad (2)$$

$$e_2 = \mathrm{L}\frac{di}{dt} \quad \dots\dots\dots \quad (3)$$

Now if we suppose that there is no loss in either the condenser or the choking coil, then by squaring (1) and taking mean values we get—

$$\mathrm{V}^2 = \mathrm{R}^2\,\mathrm{C}^2 + \mathrm{V}_1^2 + \mathrm{V}_2^2 + 2\,\mathrm{V}_1\,\mathrm{V}_2 \cos\phi \quad \dots \quad (4)$$

where C is the effective value of i and ϕ is the phase difference between e_1 and e_2.

By definition—

$$\cos\phi = \frac{\frac{1}{\mathrm{T}}\int_0^{\mathrm{T}} e_1\,e_2\,dt}{\mathrm{V}_1\,\mathrm{V}_2}$$

From (2) and (3)—

$$\frac{1}{\mathrm{T}}\int_0^{\mathrm{T}} e_1\,e_2\,dt = \frac{\mathrm{K}\,\mathrm{L}}{\mathrm{T}}\int_0^{\mathrm{T}} e_1\frac{d^2 e_1}{dt^2}\,dt$$

$$= \frac{\mathrm{K}\,\mathrm{L}}{\mathrm{T}}\,\overline{e_1\frac{d e_1}{dt}}\Bigg|_0^{\mathrm{T}} - \frac{\mathrm{K}\,\mathrm{L}}{\mathrm{T}}\int_0^{\mathrm{T}}\left(\frac{d e_1}{dt}\right)^2 dt$$

$$= -\frac{\mathrm{L}\,\mathrm{C}^2}{\mathrm{K}}$$

$$\therefore\ \mathrm{Cos}\,\phi = -\frac{\mathrm{L}\,\mathrm{C}^2}{\mathrm{K}\,\mathrm{V}_1\,\mathrm{V}_2} \quad \dots\dots\dots \quad (5)$$

Now the relations between V_1 and C and between V_2 and C can be expressed as follows:—

$$\mathrm{C} = \alpha\,\mathrm{V}_1\,\mathrm{K}\,f \text{ and } \mathrm{C} = \frac{\mathrm{V}_2}{\beta\,\mathrm{L}\,f}$$

wher[illegible] are constants that depend on the shape of

the current wave. Substituting these values of V_1 and V_2 in (5) we find that—

$$\cos\phi = -\frac{\alpha}{\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

Hence substituting in (4)—

$$\frac{V^2}{C^2} = R^2 + \frac{1}{\alpha^2 K^2 f^2} + \beta^2 L^2 f^2 - 2\frac{L}{K} \quad . \quad . \quad . \quad (7)$$

This is the formula that gives us the impedance of the circuit A C (Fig. 1) when we know the shape of the current wave.

Numerical Examples.

For a sine curve $\alpha = \beta = 2\pi$ and ϕ is $180°$.

For a parabolic current wave $\alpha = 2\sqrt{\frac{168}{17}}$, $\beta = \sqrt{40}$ and $\phi = 173° 46'$.

For a triangular current wave $\alpha = \sqrt{40}$, $\beta = \sqrt{48}$ and $\phi = 155° 54'$.

In (7) if we suppose K infinite it is the same as short circuiting the condenser, and we find the following expressions for the impedance I of an inductive coil (R, L).

For a parabolic current wave—

$$I^2 = R^2 + 1{\cdot}013\,(2\pi f L)^2$$

For a triangular current wave—

$$I^2 = R^2 + 1{\cdot}216\,(2\pi f L)^2$$

It is to be noted, however, that when the shape of the applied P.D. wave is fixed, the impedance can only be expressed in the form $\sqrt{R^2 + \gamma(2\pi f L)^2}$ where γ is a constant in a special case. In general it is a very complicated function of R, L and F.[1]

When the circuit (Fig. 1) is adjusted for resonance

$$(2\pi f)^2 L K = 1$$

In this case (7) becomes—

$$I^2 = R^2 + \left(\frac{4\pi^2}{\alpha^2} + \frac{\beta^2}{4\pi^2} - 2\right)(2\pi f L)^2$$

[1] *The Electrical Review*, vol. xlv. p. 744, "The Current Produced in an Inductive Coil by a Parabolic Wave of E.M.F."

For a parabolic current—

$$I^2 = R^2 + 0{\cdot}0119\,(2\,\pi f L)^2$$

For a triangular current—

$$I^2 = R^2 + 0{\cdot}2028\,(2\,\pi f L)^2$$

It is easy to see, geometrically, the values that V_1 and V_2 can have for a given applied voltage, and for a given shape of the current wave. In order to avoid solid geometry we will suppose that the resistance of the circuit is zero. In Fig. 2, if O A be the vector representing the condenser voltage, and O B represent the choking coil voltage, then the diagonal O C of the parallelogram constructed on them as adjacent sides gives the effective value of the applied voltage. The larger the angle B O A, that is, the more nearly the current wave approaches the sine curve in shape,

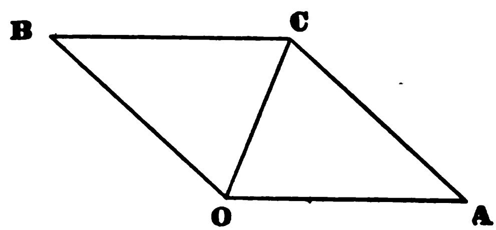

FIG. 2.

the smaller is the ratio of the resultant to its components, and hence the larger are the effects due to resonance.

Suppose (Fig. 3) that O P represents the magnitude of the voltage applied to the circuit shown in Fig. 1. Suppose also that the shape of the voltage wave is altered as the capacity or the inductance is altered, so that the current wave has always the same shape, then the condenser P.D. vectors can be measured along O A and the choking coil P.D. vectors along O B, the angle B O A remaining constant. If P R and P L be parallel to O B and O A respectively, then O R will be the condenser voltage and O L the choking coil voltage corresponding to the applied voltage O P.

Suppose that the voltage applied to the circuit is kept constant, then the locus of P is a circle. When the capacity is zero P is on O A. As the capacity increases, the

P.D. across the terminals of the condenser increases and attains its maximum value O S when the angle Q O B is a right angle. In this case

$$V_2 = -V_1 \cos\phi$$
$$\therefore KL(\beta/p)^2 = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (8)$$

Again, the choking coil voltage V_2 attains its maximum value when the angle A O S is a right angle, and in this case—

$$KL(\alpha/p)^2 = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (9)$$

The maximum value of either V_1 or V_2 is—

$$\frac{V}{\sin\phi} \text{ i.e. } V\frac{\beta}{\sqrt{\beta^2 - \alpha^2}}.$$

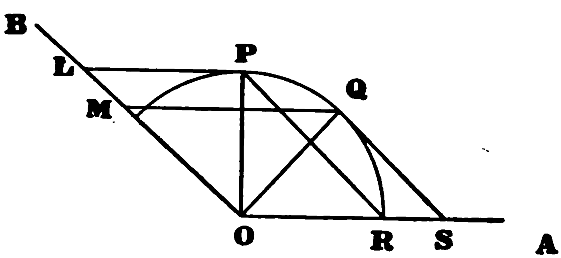

FIG. 3.

Substituting in this formula the values of α and β given above for parabolic and triangular waves, we find that for a parabolic current wave the maximum possible value of the condenser voltage is 9·22 V, where V is the applied voltage. Similarly for a triangular current wave the maximum value of the condenser voltage is 2·45 V.

For very distorted waves then it will be seen that the danger of getting excessive voltages from resonance is not nearly as great as for approximate sine waves.

A resonant circuit can only have a power-factor of unity when the shape of the applied P.D. wave is a sine curve.

It is proved in the *Electrician* for Nov. 3, 1899, p. 49, that the power-factor of a circuit can only be unity when $\frac{e}{i}$ is constant at every instant. If the resistance of the

circuit is constant then this ratio equals R, and hence from (1), (2), and (3)

$$L\frac{di}{dt} + \frac{\int i\,dt}{K} = 0$$

$$\therefore\ i = A \sin\left(\frac{2\pi t}{T} + B\right)$$

where A and B are constants and T equals $2\pi\sqrt{LK}$. Hence since $e = R\,i$ the applied P.D. wave is also a sine curve.

Since

$$C = \alpha V_1 K f = \frac{V_2}{\beta L f}$$

$$\therefore \frac{V_1}{V_2} K L (2\pi f)^2 = \frac{4\pi^2}{\alpha\beta}$$

Now when the circuit is adjusted for resonance

$$K L (2\pi f)^2 = 1$$

Hence

$$\frac{V_1}{V_2} = \frac{4\pi^2}{\alpha\beta} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (10)$$

The ratio of V_1 to V_2 is a measure of how much the current wave differs from a sine wave. If this ratio equals unity, the current wave is a sine curve, and the smaller the ratio the more distorted is the current wave.

In Fig. 4 we have supposed that the curve (I), which is a parabola, represents the shape of the wave of current. E_1 gives the shape of the wave of P.D. at the terminals of the condenser, and it is very similar to a sine curve. E_2 is the wave of P.D. at the terminals of the choking coil, and is triangular in shape. E is the wave of applied P.D. required to produce a parabolic current, and it will be seen that it is a peaky wave, very different from a sine curve. The diagram illustrates the general theorem that except when the applied P.D. wave is a sine curve, the wave of P.D. across the choking coil terminals is much more distorted from the sine shape than the wave of P.D. across the terminals of the condenser.

…e of Currents.—If we have (Fig. 5) a condenser

(K) shunted by a choking coil (L), then in certain cases the current in the main can be very small compared to either the choking coil or condenser current. Let c, i_1

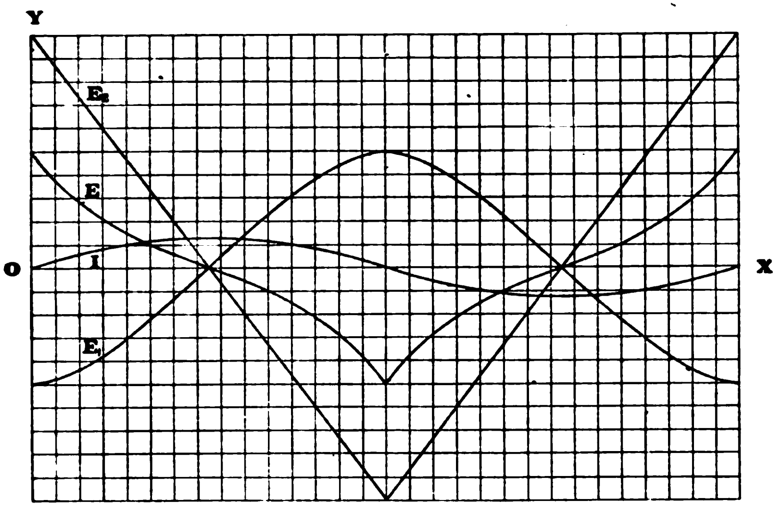

FIG. 4.

and i_2 be the instantaneous values of the P.D., the current in the condenser and the current in the choking coil, then—

$$i_1 = K\frac{dc}{dt}\,;\; e = L\frac{di_2}{dt}$$

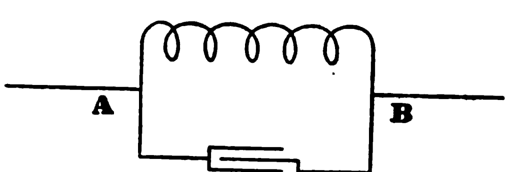

FIG. 5.

Hence—

$$i_1 = K\,L\frac{d^2 i_2}{dt^2}$$

Now if C_1 and C_2 be the effective values of i_1 and i_2 we know that—

$$C_1 = \alpha\, V\, K f \text{ and } C_2 = \frac{V}{\beta\, L f}$$

where α and β are constants depending on the shape of the wave of P.D. Values of α and β for many curves are giv

in the *Journal*, vol. xxix., p. 154. If ϕ be the phase difference between the vector values of i_1 and i_2 then it is easy to show as before that—

$$\cos\phi = -\frac{K}{L}\,\frac{V^2}{C_1 C_2}$$
$$= -\frac{\beta}{\alpha}$$

Hence ϕ only depends on the shape of the wave of the applied P.D. For very distorted waves ϕ is not much greater than 90 deg.

In Fig. 3, if O B represent the condenser current and O A the choking coil current, then O C will give the current in the main. If the capacity of the condenser is fixed, then the minimum value of the main current is $C_1 \sin\phi$, and if C_2 be the choking coil current, then in this case

$$C_2 = -C_1 \cos\phi$$
$$\therefore K L (\beta f)^2 = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (11)$$

If the inductance of the choking coil be fixed, then the minimum value of the current in the main is $C_2 \sin\phi$, and it has this value when,

$$K L (\alpha f)^2 = 1 \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (12)$$

Numerical Examples.

1. For a sine wave, ϕ is 180 deg. and the main current is zero when

$$K L (2\pi f)^2 = 1.$$

2. For a parabolic wave ϕ is 173° 46′, and if the condenser current is constant and equal to C_1 the minimum value of the current in the main is 0·1086 C_1 and it has this value when

$$1{\cdot}001\ K L (2\pi f)^2 = 1.$$

Similarly if the current in the choking coil (C_2) be kept constant, the minimum value of the main current is 0·1086 C_2, and it has this value when

$$1{\cdot}013\ K L (2\pi f)^2 = 1.$$

3. For a triangular wave ϕ is 155° 54′. When the condenser current varies the minimum value of the main current is 0·4083 C_1, and in this case

$$1{\cdot}013\ K L (2\pi f)^2 = 1.$$

When the choking coil current varies, the minimum value of the main current is $0{\cdot}4083\, C_2$, and we then have—

$$1{\cdot}216\, K L (2 \pi f)^2 = 1.$$

Test for distortion of P.D. wave.—If a condenser shunted by a choking coil be adjusted so that—

$$K L (2 \pi f)^2 = 1$$

then

$$\frac{C_1}{C_2} = \frac{\alpha \beta}{4 \pi^2}$$

When C_1 is equal to C_2 the applied P.D. is sine-shaped, and the greater this ratio the more distorted from the sine shape will be the wave of P.D. Also if C be the current in the main, the smaller C is compared to either C_1 or C_2, the nearer is the shape of the applied wave to that of a sine curve.

Conclusions.—

(1) The power-factor of a resonant circuit can only be unity when the applied P.D. wave is sine-shaped.

(2) The rise of pressure in a resonant circuit depends on the shape of the current wave. If the ohmic resistance of the choking coil and leads is negligible, the maximum pressure across the condenser would be infinite for a sine wave, 9·22 times the applied pressure for a parabolic wave, and 2·45 times the applied pressure for a triangular wave.

(3) If we vary the inductance of the choking coil, then the pressure across the condenser terminals is a maximum when $K L (\beta f)^2$ equals unity, where β is a constant that depends on the shape of the current wave. The minimum value of β is 2π, and it has this value for a sine wave.

(4) If we shunt a choking coil with a variable condenser, then the minimum current in the main is got when $K L (\alpha f)^2$ equals unity, where α is a constant that depends on the shape of the P.D. wave.

When the P.D. wave is sine-shaped α equals 2π its minimum value.

(5) A condenser and a choking coil whose magnitudes are connected by the relation

$$L\,K\,(2\,\pi f)^2 = 1$$

can be used as follows to test how much the applied P.D. wave varies from the sine shape. Connect them in parallel and let the current in the main be C, in the condenser C_1, and in the choking coil C_2. Then the smaller the ratio of C to either C_1 or C_2 the more nearly does the form of the wave approach the sine shape. If C_1 equals C_2, then the P.D. is sine-shaped, and the greater the ratio of C_1 to C_2 the more distorted the wave.

(6) If the condenser and the choking coil be connected in series, and if $L\,K\,(2\,\pi f)^2$ equals unity, then if the voltage across the choking coil (V_2) be equal to the voltage across the condenser (V_1) the current wave is sine-shaped. The greater the ratio of V_2 to V_1, the more distorted will be the current wave.

NOTICE.

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